Agricultural Research Institute
PUSA

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCIX.

PART I.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

be, the importance and singularity of the subjects, or the advantageous manner of treating them, without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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APPENDIX.

Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Medal on Sir GODFREY COPLEY's Donation, for the year 1808, to WILLIAM HENRY, M. D. for his various Papers communicated to the Society, and printed in the Philosophical Transactions.

And they adjudged the Gold and Silver Medals, on the Donation of BENJAMIN COUNT of RUMFORD, to Mr. WILLIAM MURDOCH, for his publication of the employment of Gas from Coal, for the purpose of Illumination.

PHILOSOPHICAL

TRANSACTIONS.

I. *The Croonian Lecture. On the Functions of the Heart and Arteries. By* Thomas Young, *M. D. For. Sec. R. S.*

Read November 10, 1808.

THE mechanical motions, which take place in an animal body, are regulated by the same general laws as the motions of inanimate bodies. Thus the force of gravitation acts precisely in the same manner, and in the same degree, on living as on dead matter; the laws of optics are most accurately observed by all the refractive substances belonging to the eye; and there is no case in which it can be proved, that animated bodies are exempted from any of the affections to which inanimate bodies are liable, except when the powers of life are capable of instituting a process, calculated to overcome those affections, by others, which are commensurate to them, and which are of a contrary tendency. For example, animal bodies are incapable of being frozen by a considerable degree of cold, because animals have the power of generating heat; but the

skin of an animal has no power of generating an acid, or an alkali, to neutralise the action of an alkaline or an acid caustic, and therefore its texture is destroyed by the chemical attraction of such an agent, when it comes into contact with it. As far, therefore, as the functions of animal life depend on the locomotions of the solids or fluids, those functions must be capable of being illustrated by the consideration of the mechanical laws of moving bodies ; these laws being fully adequate to the explanation of the connexion between the motive powers, which are employed in the system, and the immediate effects, which they are capable of producing, in the solids or fluids of the body : and it is obvious, that the inquiry, in what manner, and in what degree, the circulation of the blood depends on the muscular and elastic powers of the heart and of the arteries, supposing the nature of those powers to be known, must become simply a question belonging to the most refined departments of the theory of hydraulics.

In examining the functions of the heart and arteries, I shall inquire, in the first place, upon the grounds of the hydraulic investigations which I have already submitted to the Royal Society, what would be the nature of the circulation of the blood, if the whole of the veins and arteries were invariable in their dimensions, like tubes of glass or of bone ; in the second place, in what manner the pulse would be transmitted from the heart through the arteries, if they were merely elastic tubes ; and in the third place, what actions we can with propriety attribute to the muscular coats of the arteries themselves. I shall lastly add some observations on the disturbances of these motions, which may be supposed to occur in different kinds of inflammations and of fevers.

When we consider the blood vessels as tubes of invariable dimensions, we may suppose, in order to determine the velocity of the blood in their different parts, and the resistances opposed to its motion, that this motion is nearly uniform, since the alternations, arising from the pulsation of the heart, do not materially affect the calculation, especially as they are much less sensible in the smaller vessels than in the larger ones, and the principal part of the resistance arises from these small vessels. We are to consider the blood in the arteries as subjected to a certain pressure, by means of which it is forced into the veins, where the tension is much less considerable; and this pressure, originating from the contractions of the heart, and continued by the tension of the arteries, is almost entirely employed in overcoming the friction of the vessels: for the force required to overcome the inertia of the blood is so inconsiderable, that it may, without impropriety, be wholly neglected. We must therefore inquire, what the magnitude of this pressure is, and what degree of resistance we can suppose to arise from the friction of the internal surface of the blood vessels, or from any other causes of retardation. The magnitude of the pressure has been ascertained by HALES's most interesting experiments on a variety of animals, and may thence be estimated with sufficient accuracy for the human body; and for determining the magnitude of the resistance, I shall employ the theorems which I have deduced from my own experiments on very minute tubes, compared with those which had been made by former observers under different circumstances; together with some comparative experiments on the motion of water and of other fluids in the same tubes.

Dr. HALES infers, from his experiments on quadrupeds of different sizes, that the blood in the human arteries is subjected to a pressure, which is measured by a column of the height of seven feet and a half: in the veins, on the contrary, the pressure appears to amount to about six inches only: so that the force which urges the blood from the greater arteries through the minuter vessels into the large veins, may be considered as equivalent to the pressure of a column of seven feet.

In order to calculate the magnitude of the resistance, it is necessary to determine the dimensions of the arterial system, and the velocity of the blood which flows through it. According to the measurements of KEILL and others, we may take $\frac{3}{4}$ of an inch for the usual diameter of the aorta, and suppose each arterial trunk to be divided into two branches, the diameter of each being about $\frac{4}{5}$ of that of the trunk, (or more accurately $1 : 1.26 = 10 - .10056 7$), and the joint areas of the sections about a fourth part greater, (or $1.2586 : 1 = 10.099896$). This division must be continued twenty nine times, so that the diameter of the thirtieth segment may be only the eleven hundredth part of an inch, that is, nearly large enough to admit two globules of the blood to pass at once. The length of the first segment must be assumed about nine inches, that of the last, the twentieth of an inch only; and supposing the lengths of the intermediate segments to be a series of mean proportionals, each of them must be about one sixth part shorter than the preceding, (or $1 : 1.961 = 10 - .07776$), the mean length of the whole forty six inches, the capacity to that of the first segment as 72.71 to 1, and consequently the weight of the blood contained in the arterial system about 9.7

pounds. It is probable that this calculation approaches sufficiently near to the truth : for the whole quantity of blood in the body being about 40 pounds, although some have supposed it only 20, others no less than 100, there is reason to believe that half of this quantity is contained in the veins of the general circulation, and that the other half is divided, nearly in equal proportions, between the pulmonary system and the remaining arteries of the body, so that the arteries of the general circulation may contain about 9 or 10 pounds. HALLER allows 50 pounds of circulating fluid, partly serous, and partly red, and supposes $\frac{1}{5}$ of this to be contained in all the arteries taken together : but in a determination which must be in great measure conjectural we cannot expect perfect accuracy : and according to HALLER's own account of the proportions of the sections of the arteries and veins, the large trunks of the veins appear to be little more than twice as capacious as those of the arteries, and the smaller branches much more nearly equal, so that we cannot attribute to the arterial system less than $\frac{1}{3}$ of the whole blood.

It may be supposed that the heart throws out, at each pulsation, that is about seventy five times in a minute, an ounce and a half of blood : hence the mean velocity in the aorta becomes eight inches and a half in a second : and the velocity in each of the succeeding segments must of course be smaller, in proportion as the joint areas of all the corresponding sections are larger than the area of the aorta : for example, in the last order of vessels, of which the diameter is the eleven hundredth of an inch, the velocity will be one ninety third of an inch : and this result agrees sufficiently well with HALES's observation of the velocity in the capillary arteries of a frog,

which was one ninetieth part of an inch only. It is true, that HALLER is disposed to question the accuracy of this observation, and to attribute a much greater velocity to the blood flowing through the capillary vessels, but he did not attempt either to measure the velocity, or to determine it by calculation: nor is this the only instance in which HALLER has been led to reason erroneously, from a want of mathematical knowledge: he may, however, have observed the particles of blood moving in the axis of a vessel with a velocity much exceeding the mean velocity of its whole contents. If we calculate upon these foundations, from the formula which I have already laid before the Society, it will appear that the resistance which the friction of the arteries would occasion, if water circulated in them instead of blood, with an equal velocity, must amount to a force equivalent to the pressure of a column of fifteen inches and a half: to this we may add about a fourth for the resistance of the capillary veins, and we may estimate the whole friction for water, at twenty inches. The only considerable part of this force is derived from the term $\frac{2.1126lv}{10^7d^{3.5}}$ in the value of f: this term increases for each successive segment in the ratio $1 : 1.49425 = 1 : n$, and the sum of the series is to the first term, as $\frac{n^{30}-1}{n-1}$ to 1. It appears also, that a very small portion only of the resistance is created in the larger vessels: thus, as far as the twentieth division, at the distance of an inch and a quarter only from the extreme capillary arteries, the pressure of a column of one twentieth of an inch only, is required for overcoming the whole friction, and at the twenty fifth division, where the artery does not much exceed the diameter of a human hair, the height to which the

water would rise, in a tube fixed laterally into the artery, is only two inches less than in the immediate neighbourhood of the heart.

In order to judge of the comparative resistance produced by fluids of different degrees of viscidity, I employed the same tubes, by means of which I had determined the friction of water, in extreme cases, for ascertaining the effect of different substances held in solution in the water : since it is impossible to make direct experiments on the blood in its natural state, on account of its tendency to coagulate : and those substances which have the power of preventing its coagulation, may naturally be supposed to produce a material change in its viscidity. The diameter of one of the tubes, which was cylindrical, was the fortieth part of an inch : the bore of the other was oval, as is usual in the finest tubes made for thermometers : the section, divided by one fourth of the circumference, gave one hundred and seventy seconds for the mean diameter. I caused some milk, and solutions of sugar of different strength, to pass through these tubes : they were all transmitted much more sparingly than water, with an equal pressure, and the difference was more considerable in the smaller than in the larger tube, as might naturally be expected both from the nature of the resistance, and from the result of GERSTNER's experiments on water at different temperatures. In the first tube the resistance to the motion of milk was three times as great as to that of water, a solution of sugar in five times its weight of water produced twice as much resistance as water ; in twice its weight, nearly four times as much as water : but in the narrower tube, the weaker solution of sugar exhibited a resistance five times as great as that of

water, which is more than twice as much as appeared in the larger tube. Hence there can be no doubt that the resistance of the internal surface of the arteries to the motion of the blood must be much greater than would be found in the case of water: and supposing it about four times as great, instead of 20 inches, we shall have 80, for the measure of a column of which the pressure is capable of forcing the blood, in its natural course, through the smaller arteries and veins, which agrees very well with HALES's estimate.

This determination of the probable dimensions of the arterial system, and of the resistances occasioned by its different parts, is in some few respects arbitrary, at the same time that it cannot be materially altered without altering either the whole quantity of blood contained in the body, the diameters of the smallest capillary vessels, the mean number of bifurcations, or the magnitude of the resistance, all of which are here assumed nearly as they have been laid down by former observers: the estimation of the length of the successive segments only is made in such a manner, as to reconcile these data with each other, by means of the experiments and calculations relating to the friction of fluids in pipes. The effect of curvature in increasing the resistance has been hitherto neglected; it can be only sensible in the larger vessels; and supposing the flexures of these to be equivalent to the circumferences of two circles, each two inches in diameter, the radius q being 1, we have $r = \frac{.0000045 p \, v^2 \, q^{\frac{4}{3}}}{q} = .0000045 \times 720 \times 64 = .207$, or about one fifth of an inch, for the additional resistance arising from this cause in the case of water, or four fifths for blood, which is a very inconsiderable part of the whole,

It might be questioned whether the experiments, which I have made, with tubes $\frac{1}{172}$ of an inch in diameter, are sufficient for determining, with accuracy, the degree in which the resistance would be increased in tubes, of which the diameter is only one sixth part as great; and it may be doubted whether the analogy, derived from these experiments, can be safely employed as a ground for asserting, that so large a portion of the arterial pressure is employed in overcoming the resistance of the very minute arteries. But it must be remembered, that these experiments are at least conclusive with respect to the arteries larger than the tubes employed in them, and even those which are a little smaller; so that the remaining pressure, as observed in experiments, can only be employed in overcoming the resistance of the minuter arteries and veins, and these observations tend therefore immediately to confirm the analogy drawn from the experiments on the motion of water. It might indeed be asserted, that the viscidity of the blood exceeds that of water in a much greater ratio than that which is here assigned; but this is rendered improbable by some experiments of HALES, in which, when the intestines were laid open, on the side opposite to the mesentery, so that many of the smaller arteries were divided, the quantity of warm water which passed through them with an equal pressure, was only about twelve times as great as that of the blood which flows through them in their natural state; and it is probable, that at least three or four times as much of any fluid must have passed through them in their divided, as in their entire state, unless we suppose that the coats of the divided vessels, like many other muscular parts, are capable of being contracted by the contact of water. In

some other experiments, it was found that a moderate degree of pressure was capable of causing water to exude so copiously through the exhalant vessels of the intestines, that it passed through the aorta with a velocity of about two inches in a second, although these vessels do not naturally allow any passage to the blood: on the other hand, it sometimes happened that very little water would pass through such channels as naturally transmitted a much larger quantity of blood: a circumstance which Dr. Hales very judiciously attributes to the oozing of the water into the cellular membrane surrounding the vessels, by means of which they were compressed, and their diameters lessened. On the whole, it is not improbable, that in some cases the resistance, opposed to the motion of the blood, may exceed that of water in a ratio somewhat greater than I have assigned; but this must be in the minutest of the vessels, while in the larger arteries the disproportion must be less: so that, however we may view the subject, it appears to be established, that the only considerable resistance which the blood experiences, occurs in the extreme capillary arteries, of which the diameter scarcely exceeds the hundredth part of an inch.

We cannot suppose that the dimensions of the sanguiferous system agree uniformly, in all its parts, with the measures which I have laid down; but the truth of the inference is not affected by these variations. For example, there may perhaps be some arteries communicating with veins, of which the diameter exceeds the eleven hundredth of an inch; but there are certainly many others which are much more minute; and the blood, or its more liquid parts, passing through these more slowly, it must move more rapidly in the former, so that the

resistance may in all be equal to the pressure, and the mean velocity may still remain such as is determined by the quantity of blood passing through the aorta. There is indeed some uncertainty in the measure of the globules of the blood, which I have made the basis of the dimensions of the minute arteries: and I have reason to think, that instead of $\frac{1}{2000}$ of an inch, their greatest diameter does not exceed $\frac{1}{3000}$, or even $\frac{1}{3600}$: the general results of the investigation are not however affected by this difference : it will only require us to suppose the subdivisions somewhat more numerous, and the branches shorter.

These are the principal circumstances which require to be considered, with respect to the simple transmission of the blood through the arteries into the veins, without regard to the alternate motions of the heart, and to the elastic and muscular powers of the vessels. I shall next examine the nature and velocity of the propagation of the pulse. The successive transmission of the pulsations of the heart, through the length of the arteries, is so analogous to the motion of the waves on the surface of water, or to that of a sound transmitted through the air, that the same calculations will serve for determining the principal affections of all these kinds of motion ; and if the water, which is agitated by waves, is supposed to flow at the same time in a continued stream, and the air which conveys a sound to be carried forwards also in the form of a wind, the similitude will be still stronger. The coats of the arteries may perhaps be considered, without much inaccuracy, as perfectly elastic, that is, as producing a force proportional to the degree in which they are extended beyond their natural dimensions; but it is not impossible that there may be some bodies in nature, which differ materially from this general

law, especially where the distension becomes considerable: thus there may be substances which exhibit a force of tension proportional to the excess of the square, or the cube of their length, beyond a certain given quantity. It is safest therefore to reason upon the elasticity of any substance, from experiments made without any great deviation from the circumstances to which the calculation is to be applied.

For this purpose, we may again employ some of the many excellent experiments contained in Hales's hæmastatics. It appears, that when any small alteration was made in the quantity of blood contained in the arteries of an animal, the height of the column, which measured the pressure, was altered nearly in the same proportion, as far as we are capable of estimating the quantity, which was probably contained in the larger vessels of the animal. Hence it follows, that the velocity of the pulse must be nearly the same as that of an impulse transmitted through an elastic fluid, under the pressure of a column of the same height, as that which measures the actual arterial pressure: that is, equal to that which is acquired by a heavy body falling freely through half this height. In man, this velocity becomes about fifteen feet and a half in a second; to which the progressive motion of the blood itself adds about eight inches; and with this velocity, of at least sixteen feet in a second, it may easily happen that the pulse may appear to arrive at the most distant parts of the body without the intervention of any very perceptible interval of time.

The velocity of the transmission of the pulse being known, it is easy to determine the degree in which the arteries are dilated during its passage through them. The mean velocity of the blood in the aorta being eight inches and a half in a

second, its greatest velocity must be about three times as much, since the contraction of the heart is supposed to occupy only about one third part of the interval between two successive pulsations ; and if the velocity of the pulse is sixteen feet in a second, that of the blood itself must be about one eighth part as great ; so that the column of blood occupying eight inches may occupy only seven ; hence the diameter must increase in the ratio of about fifteen to sixteen. The tension will also become one eighth greater, and the force of the heart must be capable of supporting a column of one hundred and one inches. This force would, however, require to be somewhat increased, from the consideration that the force required at the end of any canal during the reflection of a pulsation or wave of any kind, is twice as great as the force exerted during its transmission, and the force employed in the origination of a wave or pulse in a quiescent fluid, is the same as is required for its reflection ; on the other hand, a weaker pulsation, proceeding into a narrower channel, becomes more energetic, so that, from this consideration, a force somewhat smaller would be required in the heart: on the whole, however, it appears probable, that the former of these corrections must be the more considerable, and that the force of the heart must be measured by the pressure of a column, rather more than less than one hundred and one inches high: nor would this force by any means require a strong exertion of muscular power; for it only implies a tension of something less than three pounds for each inch of the circumference of the greatest section of the heart; and supposing the mean thickness half an inch, an equal number of the fibres of some other muscles of the body would be capable of exerting a force of

more than two hundred pounds, in the state of the greatest possible action.

The force, here assigned to each pulsation, agrees extremely well with the inference that may be drawn from an experiment of HALES, on the ascent of the blood in a tube connected with an artery of a horse. The whole height of the column being nine feet, the blood rose about three inches higher during each pulsation, which was repeated fifty or sixty times in a minute: now we may suppose the acceleration to have extended a little beyond the first half of the space thus described, so that two inches were described in two fifths of a second; and if there had been no friction, nor any other cause of retardation, there can be no doubt that at least four inches would have been described in the same time; but the same column of nine feet, if it had been actuated by its own weight, would have described thirty one inches in the same time: consequently the force with which the blood was forced through the artery was nearly one eighth of the whole force of tension, as it appears in the former calculation.

The magnitude of the pulse must diminish in the smaller arteries in the subduplicate proportion of the increase of the joint areas, in the same manner as the intensity of sound is shewn to decrease in diverging from a centre, in the subduplicate ratio of the quantity of matter affected by its motion at the same time. For example, in the arteries of the tenth order, of which the diameter is one thirteenth of an inch, its magnitude must be only one third as great as in the aorta, that is, the greatest progressive velocity of the blood must be eight inches and a half in a second only, and the dilatation one fiftieth part only of the diameter. In the vessels of the

twentieth order, the dilatation does not exceed $\frac{1}{160}$ of the diameter, which is itself the 140th part only of an inch : so that it is not surprising, that HALLER should have been unable to discover any dilatation in vessels of these dimensions, even with the assistance of a powerful microscope. If we estimated the magnitude of the pulse in the aorta, from the excess of the temporary above the mean velocity, which would perhaps be justifiable, that magnitude would become still less considerable.

These calculations agree extremely well with each other, and with experiment, as far as they relate to the power of the heart, and the affections of the smaller arteries. But there is reason to think that the velocity of the pulse in the larger vessels is much more considerable than has been here stated ; and their dilatation is also less conspicuous, when they are exposed to view, than it would probably be, if it were as great as is inferred from the velocity here assigned. I have demonstrated, in the hydraulic investigations which I lately laid before the Royal Society, that the velocity of an impulse passing through a tube, consisting of perfectly elastic materials, is half as great as that of a body supposed to have fallen from the given point to the base of the modular column of the tube : and that the height of this column is such that the tube would be extended without limit by its pressure ; consequently it must be greater than the height of a column equivalent to the pressure by which the tube is burst. Now it has been ascertained by Dr. HALES, that the pressure, required for bursting one of the carotids of a dog, is equal to that of a column of water one hundred and ninety feet high ; nor does he remark that the artery was very materially dilated ; and deducting from this height the five feet which express the

actual pressure in the arteries of a dog, the remaining one hundred and eighty five feet will give a velocity of at least fifty four feet in a second, for the propagation of the pulse in the dog. It is not however ascertained, that all the membranes, which may have surrounded the artery in this experiment, are called into action in its ordinary pulsation, much less that the force, developed by their tension, varies precisely according to the general law of perfectly elastic bodies : but this mode of calculation is still amply sufficient to make it probable, that the velocity of the pulsations, in the larger arteries, must amount to at least forty feet in a second, although some very considerable deductions must be made, on account of the resistances of various kinds, which cannot be comprehended in the calculation.

The artery must not be supposed to subside, immediately after each pulsation, precisely to its original dimensions, since it must remain somewhat fuller, in order to supply the capillary arteries, and the veins, in the interval between the two successive pulsations : and in this respect it differs from the motions of a wave through a canal, which is open on both sides : but the difference may be understood, by supposing a partial reflection of the pulse to take place at every point where it meets with any resistance, which will leave a general distension of the artery, without any appearance of a retrograde pulsation.

I shall proceed to inquire, in the third place, into the nature and extent of the functions which are to be attributed to the muscular fibres of the coats of the arteries; and I apprehend that it will appear to be demonstrable, that they are much less concerned in the progressive motion of the blood, than

is almost universally believed. The arguments, which may be employed to prove this, are nearly the same that I have already stated, in examining the motion of a fluid, carried along before a moving body in an open canal; but in the case of an elastic tube, the velocity of the transmission of an impulse being rather diminished than increased by an increase of tension, the reasoning is still stronger and simpler; for it may here be safely asserted, that the anterior parts of the dilatation, which must be forced along by any progressive contraction of the tube, can only advance with the velocity appropriate to the tube, and that its capacity must be proportionate to its length and to the area of its section : now the magnitude of its section must be limited by that degree of tension which is sufficient to force back through the contraction what remains of the displaced fluid, and the length by the difference of the velocity appropriate to the tube, and that with which the contraction advances ; consequently if the contraction advance with the velocity of a pulsation, as any contractile action of the arteries must be supposed to do, this length necessarily vanishes, and with it the quantity of the fluid protruded ; the whole being forced backwards, by the distending force which is exerted by a very small dilated portion, immediately preceding the contraction. It might indeed be imagined, that the contraction follows the pulsation with a velocity somewhat smaller than its own; but this opinion would stand on no other foundation than mere conjecture, and it would follow, that the pulse would always become more and more full, as it became more distant from the heart; of which we have nothing like evidence: nor would a moderate contraction, even if this supposition were granted, produce any material effect. For

example, if the velocity of the contraction were only half as great as that of the pulsation, which is the most favourable proportion, it would be necessary, taking sixteen feet in a second for the velocity of the pulsation, that the section of the arteries should be contracted to about one half, in order to produce, by their progressive contraction only, the actual velocity of the blood in the aorta; one sixteenth of the blood being carried, in this case, before the contraction: but if the contraction were only such, as to reduce the section of the artery to $\frac{9}{10}$, which is probably more than ever actually happens, the velocity produced would be only about $\frac{1}{80}$ as much; and if the contraction were only to $\frac{99}{100}$, which is a sufficient allowance for the smaller arteries, about $\frac{1}{10000}$ only of the actual velocity in the aorta could be produced in this manner, even upon a supposition much more favourable to the muscular action of the arteries than the actual circumstances. A small addition must be made to the force required for producing the retrograde motion, on account of the friction to be overcome, but the general reasoning is not affected by this correction.

The contraction of the artery might also be supposed to remain after each pulsation, so that the vessel should not be again dilated until the next pulsation, or, in other words, a spontaneous dilatation might be supposed to accompany the pulsation, instead of a contraction : but such a dilatation would be useless in promoting the progressive motion of the blood, since a larger quantity of blood, conveyed to the smaller vessels, without an increased tension, would be ineffectual with respect to the resistances which are to be overcome. It is possible indeed that the muscular fibres of those arteries in which the magnitude of the pulse is sensible, like the fibres of

the heart, may be inactive, or nearly so, during their dilatation, and that they may contract after they have been once distended, with a force which is in a certain degree permanent; the greater momentum of the blood, which accompanies the dilatation, enabling it to enter the minute arteries with equal ease, although assisted by a tension somewhat smaller : so that the same mean velocity may be sustained, as if the arteries were simply elastic, and a little smaller in diameter, with a very little less exertion of the heart. But the distribution of the blood could never be materially diversified by any operation of this kind : for if any artery were for a moment distended by such a variation, so as to exceed its natural diameter by one hundredth part only, a pressure would thence arise equivalent to that of a column about two inches high, which would, in spite of all resistances, immediately dissipate the blood with a considerable velocity, and completely prevent any local accumulation, unless the elastic powers of the vessel itself were diminished; and this is, perhaps, the most important, as well as the best established inference from the doctrine that I have advanced.

It appears that a mola has sometimes been found in the uterus, totally destitute of a heart, in which the blood must have circulated in its usual course through the veins and arteries : in this case it cannot be ascertained whether there was any alternate pulsation, or whether the blood was carried on in a uniform current, in the same manner as the sap of a vegetable probably circulates. If there was a pulsation, it may have been maintained by a contraction of the artery, much more considerable, and slower in its progress than usual; and with the assistance of a spontaneous dilatation; the resistance

in the extreme vessels being also probably much smaller than usual : if the motion was continued, it would lead us to imagine that there may be some structure in the placenta capable of assisting in the propulsion of the blood, as there may possibly be some arrangement in the roots of plants by which they are calculated to promote the ascent of the sap. The circulation in the vessels of the more imperfect animals, in which a great artery supplies the place of a heart, is of a very different nature from that of the more perfect animals : the great artery, which performs the office of the heart, is here possessed of a muscular power commensurate to its functions, and seems to propel the blood, though much more slowly than in other cases, by means of a true peristaltic motion. It appears also from the observations of SPALLANZANI, that in many animals a portion of the aorta, next the heart, is capable of exhibiting a continued pulsation, even when perfectly empty and separated from the heart ; but this property is limited to a small part of the artery only, which is obviously capable of being essentially useful in propelling the blood when the valves of the aorta are closed. The muscular power of the termination of the vena cava is also capable of assisting the passage of the blood into the auricle. It is not at all improbable that a muscle of involuntary motion, which had been affected throughout the whole period of life by alternate contractions and relaxations, might retain from habit the tendency to such contractions, even without the necessity of supposing, that the habit was originally formed for a purpose to be obtained by the immediate exertion of the muscular power : but in fact the partial pulsation of the vena cava is perfectly well calculated to promote the temporary repletion of the

auricle, while it must retard, for a moment, the column which is approaching, at a time that it could not be received.

There is no difficulty in imagining what services the muscular coats of the arteries may be capable of performing, without attributing to them any immediate concern in supporting the circulation. For since the quantity of blood in the system is on many accounts perpetually varying, there must be some means of accommodating the blood vessels to their contents. This circumstance was very evident in some of HALES's experiments, when after a certain quantity of blood had been taken away, the height of the column, which measured the tension of the vessels, frequently varied in an irregular manner, before it became stationary at a height proportional to the remaining permanent tension. HALLER also relates, that he has frequently seen the arteries completely empty, although in some of his observations there was probably only a want of red globules in the blood which was flowing through them. Such alterations in the capacity of the different parts of the body are almost always to be attributed to the exertion of a muscular power. A partial contraction of the coats of the smaller arteries may also have an immediate effect on the quantity of blood contained in any part, although very little variation could be produced in this manner by a change of the capacity of the larger vessels.

According to this statement of the powers which are concerned in the circulation, it must be obvious that the nature of the pulse, as perceptible to the touch, must depend almost entirely on the action of the heart, since the state of the arteries can produce very little alteration in its qualities. The greater or less tension of the arterial system may indeed render

the artery itself, when at rest, somewhat harder or softer; and, if the longitudinal fibres give way to the tending force, it may become also tortuous : possibly too a very delicate touch may in some cases perceive a difference in the degree of dilatation, although it is seldom practicable to distinguish the artery, in its quiescent state, from the surrounding parts. But the sensation, which is perceived when the artery is compressed, as usual, by the finger, is by no means to be confounded with the dilatation of the artery ; for in this case an obstacle is opposed to the motion of the blood, against which it strikes, with the momentum of a considerable column, almost in the same manner as a stream of water strikes on the valve of the hydraulic ram ; and in this manner, neglecting the difference of force arising from the different magnitudes of the sections, the pressure felt by the finger becomes nearly equal and similar to that which is originally exerted by the heart: each pulsation passing under the finger, in the same time, as is required for the contraction of the heart, although a very little later; and more or less so, in proportion as the artery is more or less distant; the artery remaining then at rest for a time equal to that in which the heart is at rest. When therefore an artery appears to throb, or to beat more strongly than usual, the circumstance is only to be explained from its greater dilatation, which allows it to receive a greater portion of the action of the heart, in the same manner as an aneurysm exhibits a very strong pulsation, without any increase of energy, either in itself, or in the neighbouring vessels ; and on the other hand, when the pulsations of the artery of a paralytic arm become feeble, we cannot hesitate to attribute the change to its permanent con-

traction, since the enlargement and contraction of the blood-vessels of a limb are well known to attend the increase or diminution of its muscular exertions. There is also another way, in which the diminution of the strength of an artery may increase the apparent magnitude of the pulse, that is, by diminishing the velocity with which the pulsation is transmitted: for we have seen that the magnitude of the pulse is in the inverse ratio of the length of the artery distended at once; and this length is proportional to the velocity of the transmission: but it must be observed, that the force of the pulse striking the finger would not be affected by such a change, except that it might be rendered somewhat fuller and softer, although a considerable throbbing might be felt in the part, from the increased distension of the temporary diameter of the artery. How little a muscular force is necessary for the simple transmission of a pulsation, may easily be shown by placing a finger on the vena saphena, and striking it with the other hand at a distant part; a sensation will then be felt precisely like that of a weak arterial pulsation.

The deviations from the natural state of the circulation, which are now to be cursorily investigated, may be either general or partial; and the general deviations may consist either in a change of the motion of the heart, or of the capacity of the capillary arteries. When the motion of the heart is affected, the quantity of blood transmitted by it may either remain the same as in perfect health, or be diminished, or increased. Supposing it to remain the same, the pulse, if more frequent, must be weaker, and if slower, it must be stronger; but this latter combination is scarcely ever observable; and in the former case, the heart must either never be filled, perhaps

on account of too great irritability, or never be emptied, from the weakness of its muscular powers. But the immediate effect of such a change as this, in the functions depending on the circulation, cannot be very material, and it can only be considered as an indication of a derangement in the nervous and muscular system, which is not likely to lead to any disease of the vital functions. When the quantity of the blood transmitted by the heart is smaller than in health, the arteries must be contracted, until their tension becomes only adequate to propel the blood, through the capillary vessels, with a proportionally smaller velocity, and the veins must of course become distended, unless the muscular coats of the arteries can be sufficiently relaxed to afford a diminished tension, which is probably possible in a very limited degree only. In this state the pulse must be small and weak, and the arteries being partly exhausted, there will probably be a paleness and chilliness of the extremities: until the blood, which is accumulated in the veins, has sufficient power to urge the heart to a greater action, and perhaps, from the vigour which it may have acquired during the remission of its exertions, even to a morbid excess of activity. Hence a contrary state may arise, in which the quantity of blood transmitted by the heart is greater than in perfect health; the pulse will then be full and strong, the arteries being distended, so as to be capable of exerting a pressure sufficient to maintain an increased velocity, and to overcome the consequent increase of resistance; a state which perhaps constitutes the hot fit of fever; and which is probably sometimes removed in consequence of a relaxation of the extreme arteries, which suffer the superfluous blood to pass more easily into the veins. Such a relaxation, when carried

to a morbid extent, may also be a principal cause of another general derangement of the circulation, the motion of the blood being accelerated, and the arteries emptied, so that the pulse may be small and weak, while the veins are overcharged, and the heart exhausted by violent and fruitless efforts to restore the equilibrium ; and this state appears to resemble, in many respects, the affections observed in typhus. When, on the contrary, the capillary vessels are contracted, the arteries are again distended, although without the excess of heat which must attend their distension from an increased action of the heart, and possibly without fever: an instance of this appears to be exhibited in the shrinking of the skin, which is frequently observable from the effect of cold, and in the first impression produced by a cold bath: nor is it impossible, that such a contraction may exist in the cold fit of an intermittent, although it seems more probable that a debility of the heart is the primary cause of this affection.

Besides these general causes of derangement, which appear to be more or less concerned in different kinds of fever, there are other more partial ones, which seem to have a similar relation to local inflammations. The most obvious of these changes are such as must be produced by partial dilatations or contractions of the capillary vessels ; since, as I have endeavoured to demonstrate, any supposed derangement in the actions of the larger vessels must be excluded from the number of causes which can materially affect the circulation. It cannot be denied, that a diminution of the elastic, or even of the muscular force of the small arteries, must be immediately followed by such a distension as will produce a resistance equal to the pressure: the distension will occasion an increase

of redness, and in most cases pain: the heat will also generally be increased, on account of the increased quantity of blood which will be allowed to pass through the part; and since the hydrostatic pressure of the blood acquires greater force, as the artery becomes more distended, it may be so weak as to continue to give way, like a ligament which has been strained, until supported by the surrounding parts. In this state a larger supply of blood will be ready for any purposes which require it, whether an injury is to be repaired, or a new substance formed; and it is not impossible, that this change in the state of the minute vessels may ultimately produce some change in the properties of the blood itself.

The more the capillary arteries are debilitated and distended, the greater will be the mean velocity of the circulation; but whether or no the velocity will be increased in the vessels which are thus distended, must depend on the extent of the affected part; and it may frequently happen that the velocity may be much more diminished on account of the dilatation of the space which the blood is to occupy, than increased by the diminution of the resistance. And on the other hand, the velocity may be often increased, for a similar reason, at the place of a partial contraction. Hence we may easily understand some of the experiments which Dr. WILSON has related in his valuable treatise on fevers: the application of spirit of wine to a part of the membrane of a frog's foot contracted the capillary arteries, and at the same time accelerated the motion of the blood in them, while in other parts, where inflammation was present, and the vessels were distended, the motion of the blood was slower than usual.

Another species of inflammation may probably be occa-

sioned by a partial constriction or obstruction of the capillary arteries, which must indeed be supposed to exist where the blood has become wholly stagnant, as Dr. WILSON in some instances found it. This obstruction must however be extended to almost all the branches, belonging to some small trunk, in which the pressure remains nearly equal to the tension of the large arteries; for in this case it will happen, that the whole pressure will be continued throughout the obstructed branches, without the subtraction of the most considerable part, which is usually expended in overcoming the resistances dependent on the velocity; so that the small branches will be subjected to a pressure, many times greater than that which they are intended to withstand in the natural state of the circulation; whence it may easily happen that they may be morbidly distended; and this distension may constitute an inflammation, attended by redness and pain. Nor is it impossible, that obstructions of this kind may originate in a vitiated state of the blood itself, although it would be difficult to prove the truth of the conjecture; it seems, however, to be favoured by the observation of HALLER, that little clots of globules may often be observed in the arteries, when the circulation is languid, and that they disappear when its vigour is restored, especially after venesection. But if a very small number only of capillary arteries be obstructed, other minute branches will still be capable of receiving the blood, which ought to pass through them, without any great distension or increase of pressure: and this exception is sufficient to explain another experiment of Dr. WILSON, in which a small obstruction, caused by puncturing a membrane with a hot needle, failed to excite an inflammation. This species of inflammation is probably

attended by less heat than the former; and where the obstruc-
tion is very great, it may perhaps lead immediately to a morti-
fication, which is called by the Germans " a cold burning."

The most usual causes of inflammation appear to be easily
reconcileable with these conjectures. Suppose any consider-
able part of the body to be affected by cold; the capillary
vessels will be contracted, and at the same time the tempera-
ture of some parts of their contents will be lowered, from both
of which causes the resistance will be increased, and the arte-
ries in general will be more or less overcharged : if then any
other part of the system be at the same time debilitated or over-
heated, its arteries will be liable to be morbidly distended, and
an inflammation may thus arise, which may continue till the
minute vessels are supported and strengthened, by means of
an effusion of coagulable lymph. The immediate effect, either
of cold or of heat, may also sometimes produce such a degree
of debility in any part, as may lay the foundation of a subse-
quent inflammation : but the first effect of heat in the blood-
vessels appears to be the more ready transmission of the blood
into the veins, by means of which they become very observ-
ably prominent : and cold, which checks the circulation in the
cutaneous vessels, probably occasions a livid hue, by retaining
the blood stagnant longer than usual in the capillary vessels
of all kinds. It may be objected, that an obstruction of the
motion of the blood through a great artery ought, upon these
principles, to produce an inflammation in some distant part :
but in this case, the blood will still find its way very copiously
into the parts supplied by the artery, by means of some col-
lateral branches, which will always admit a much larger
quantity of blood than usually passes through them, whenever

a very slight excess of force can be found to carry it on, or when the blood which they contain finds a readier passage than usual, by means of their communication with such parts as are now deprived of their natural supply.

It is difficult to determine, whether blushing is more probably effected by a constriction or by a relaxation of the vessels concerned; it must, however, be chiefly an affection of the smaller vessels, since the larger ones do not contain a sufficient quantity of blood to produce so sudden an effect. Perhaps the capillary vessels are dilated, while the arteries, which are a little larger only, are contracted : possibly too an obstruction may exist at the point of junction of the arteries with the veins; and where the blush is preceded by paleness, such an obstruction is probably the principal cause of the whole affection.

With respect to the tendency of inflammation in general to extend itself to the neighbouring parts, it is scarcely possible to form any reasonable conjecture that can lead to its explanation: this circumstance appears to be placed beyond the reach of any mechanical theory, and to belong rather to some mutual communication of the functions of the nervous system, since it is not inflammation only, that is thus propagated, but a variety of other local affections of a specific nature, which are usually complicated with inflammation, although they may perhaps, in some cases, be independent of it. Inflammations, however, are certainly capable of great diversity in their nature, and it is not to be expected, that any mechanical theory can do more than to afford a probable explanation of the most material circumstances, which are common to all the different species.

Besides these general illustrations of the nature of fevers and inflammations, the theory which has been explained may sometimes be of use, in enabling us to understand the operation of the remedies employed for relieving them. Thus it may be shown, that any diminution of the tension of the arterial system must be propagated from the point at which it begins, as from a centre, nearly in the same manner, and with the same velocity, as an increase of tension, or a pulsation of any kind would be propagated. Hence the effect of venesection must be not only more rapidly, but also more powerfully felt in a neighbouring than in a distant part : and although the mean or permanent tension of the vessels of any part must be the same, from whatever vein the blood may have been drawn, provided that they undergo no local alteration, yet the temporary change, produced by opening a vein in their neighbouhood, may have relieved them so effectually from an excess of pressure, as to allow them to recover their natural tone, which they could not have done without such a partial exhaustion of the neighbouring vessels. But since it seems probable, that the minute arteries are more affected by distension than the veins, there is reason in general to expect a more speedy and efficacious relief in inflammations, from opening an artery than a vein : this operation, however, can seldom be performed without material inconvenience ; but it is probably for a similar reason, that greater benefit is often experienced from withdrawing a small portion of blood by means of cupping or of leeches, than a much larger quantity by venesection, since both the former modes of bleeding tend to relieve the arteries, as immediately as the veins, from that distension, which appears to constitute the most essential characteristic of

inflammation. In a case of hemorrhage from one of the sinuses of the brain, a very judicious physician lately prescribed the digitalis: if the effect of this medicine tends principally to diminish the action of the heart, as is commonly supposed, it was more likely to be injurious than beneficial, since a venous plethora must be increased by the inactivity of the heart; but if the digitalis diminishes the general tension of the arteries, in a greater proportion than it affects the motion of the heart, it may possibly be advantageous in venous hemorrhages. We have, however, no sufficient authority for believing, that it has any such effect on the arterial system in general.

Although the arguments, which I have advanced, appear to me sufficient to prove, that, in the ordinary state of the circulation, the muscular powers of the arteries have very little effect in propelling the blood, yet I neither expect nor desire that the prevailing opinion should at once be universally abandoned. I wish, however, to protest once more against a hasty rejection of my theory, from a superficial consideration of cases, like that which has been related by Dr. CLARKE; and to observe again, that the objections, which I have adduced, against the operation of the muscular powers of the arteries in the ordinary circulation, not being applicable to these cases, they are by no means weakened by any inferences which can be drawn from them.

ERRATA.

In the last volume of the Philosophical Transactions, page 183, line 25, for $\frac{c}{1}$, read $\frac{c}{2}$: page 184, at the end, add, is denoted by av: page 186, line 4, for when *ced*, read whence *d*.

II. *An Account of some Experiments, performed with a View to ascertain the most advantageous Method of constructing a Voltaic Apparatus, for the Purposes of Chemical Research.* By John George Children, *Esq. F. R. S.*

Read November 24, 1808.

THE late interesting discoveries by Mr. DAVY, having shewn the high importance of the VOLTAIC battery, as an instrument of chemical analysis, it became a desirable object to ascertain that mode of constructing it, by which the greatest effect may be produced, with the least waste of power and expense.

For this purpose, I made a battery, on the new method, with plates of copper and zinc, connected together by leaden straps, soldered on the top of each pair of plates; which are twenty in number, and each plate four feet high, by two feet wide: the sum of all the surfaces being 92160 square inches, exclusive of the single plate at each end of the battery. The trough is made of wood, with wooden partitions well covered with cement, to render them perfectly tight, so that no water can flow from one cell to another. The battery was charged with a mixture of three parts fuming nitrous, and one part sulphuric acid, diluted with thirty parts of water, and the quantity used was 120 gallons.

In the presence, and with the kind assistance of Messrs. DAVY, ALLEN, and PEPYS, the following experiments were made.

Experiment 1. Eighteen inches of platina wire, of $\frac{1}{30}$th of an inch diameter, were completely fused in about twenty seconds.

Exp. 2. Three feet of the same wire were heated to a bright red, visible by strong day-light.

Exp. 3. Four feet of the same wire were rendered very hot; but not perceptibly red by day-light. In the dark, it would probably have appeared red throughout.

Exp. 4. Charcoal burnt with intense brilliancy.

Exp. 5. On iron wire, of about $\frac{1}{70}$th of an inch diameter, the effect was strikingly feeble. It barely fused ten inches, and had not power to ignite three feet.

Exp. 6. Imperfect conductors were next submitted to the action of the battery, and barytes, mixed with the red oxyde of mercury, and made into a paste with pipe-clay and water, was placed in the circuit; but neither on this, nor on any other similar substance was the slightest effect produced.

Exp. 7. The gold leaves of the electrometer were not affected.

Exp. 8. When the cuticle was dry, no shock was given by this battery, and even though the skin was wet, it was scarcely perceptible.

Before I offer any observations on the inferences to be drawn from these experiments, I shall mention some others, performed, for the sake of comparison, with the foregoing, with an apparatus very different in size and number of plates, from the one just described.

This second battery was precisely the *Couronne des Tasses* of Sig. VOLTA, consisting of two hundred pairs of plates, each about two inches square, placed in half pint pots of common

queen's ware, and made active by some of the liquor used in exciting the large battery, to which was added a fresh portion of sulphuric acid, equal to about a quarter of a pint to a gallon.

To state as shortly as possible the effects produced by this battery :

Experiment 1. It decomposed potash and barytes readily.

Exp. 2. It produced the metallization of ammonia with great facility.

Exp. 3. It ignited charcoal vividly.

Exp. 4. It caused considerable divergence of the gold leaves of the electrometer.

Exp. 5. It gave a vivid spark, after being in action three hours. At the expiration of twenty-four hours, it retained sufficient power to metallize ammonia, and continued, with gradually decreasing energy, to produce the same effect, till the end of forty-one hours, when it seemed *nearly* exhausted.

From the results of the foregoing experiments, which though simple and not numerous, I trust, are satisfactory ; we see Mr. DAVY's theory of the mode of action of the VOLTAIC battery confirmed : he says (in his Paper on some Chemical Agencies of Electricity, Sect. 9. after having shewn the effect of induction to increase the electricity of the opposite plates), " the *intensity* increases with the *number*, and the *quantity* with the *extent* of the series."

That this is so, the effects produced on the platina and iron wires, in the first and fifth experiments with the large battery, and the subsequent experiments on imperfect conductors, with the small apparatus, sufficiently prove. The platina wire being a perfect conductor, and not liable to be oxydated, presents no obstacle to the free passage of the electricities through it,

which, from the immense quantities given out from so large a surface, evolve, on their mutual annihilation, heat sufficient to raise the temperature of the platina to the point of fusion.

With the iron wire, of $\frac{1}{70}$th of an inch diameter, the effect is very different, which is explained by the low state of the intensity of the electricity (sufficiently proved by its not causing any divergence of the gold leaves of the electrometer), which being opposed in its passage by the thin coat of oxide, formed on the iron wire, at the moment the circuit is completed, a very small portion only of it is transmitted through the wire. To the same want of intensity is to be attributed the total inability of the large battery to decompose the barytes, and its general weak action on bodies which are not perfect conductors. The small battery, on the contrary, exerts great power on imperfect conductors, decomposing them readily, although its whole surface is more than thirty times less than that of the great battery; but in point of number of plates, it consists of nearly ten times as many as the large one. The long continued action of the small battery, proves the utility of having the cells of sufficient capacity to hold a large quantity of liquor, by which much trouble of emptying and filling the troughs is avoided, and the action kept up, without intermission, for a long space of time, a circumstance, in many experiments, of material consequence. Besides this advantage, *with very large combinations*, a certain distance between each pair of plates is *absolutely necessary*, to prevent spontaneous discharges, which will otherwise ensue, accompanied with vivid flashes of electric light, as I have experienced, with a battery of 1250 four-inch plates, on the new construction. And here I beg leave to mention an experiment, which, though not

directly in point, cannot be considered as foreign to the sub-
ject of this Paper. It has been urged, as one proof of the
non-identity of the common electricity, and that given out by
the VOLTAIC apparatus, that in the latter there is no striking
distance. That objection, however, must cease. I took a small
receiver, open at one end; through perforations in the oppo-
site sides of which were placed two wires, with platina points,
well polished : one was fixed by cement to the glass, the other
was moveable, by means of a fine screw, through a collar of
leathers, and the distance between the points was ascertained
by a small micrometer attached. This receiver was inverted
over well dried potash over mercury, and suffered to stand a
couple of days, to deprive the air it contained, as thoroughly
as possible, of moisture. The 1250 plates being excited pre-
cisely to the same degree as the great battery, mentioned in
the beginning of this communication; and the little receiver
placed in the circuit, I ascertained its striking distance to be
$\frac{1}{50}$th of an inch. That I might be certain that the air in the
apparatus had not become a conductor by increase of temper-
ature, I repeated the experiment several times with fresh cool
air, and always with the same result; but perhaps it will be
objected, that the striking distance was so small, as not to
afford a satisfactory refutation of the argument alluded to,
when it is considered to how very great a distance, compara-
tively, the spark of the common electrical machine can pass
through air. The answer to this is obvious : increase the
number of the plates, and the striking distance will increase ;
for we see throughout, the intensity proportioned to the num-
ber, and it probably may be carried to such extent, as even to
pass through a thicker plate of air, than the common spark.

The great similarity of the appearance of the electric light of this battery in vacuo, and that of the common machine, might also be urged as an additional proof of the identity of their nature.

The effect of this large combination on imperfect conductors, was, as may be supposed, very great; but of the same platina wire, of which the four-feet plates fused eighteen inches, this battery melted but half an inch, though, had the effect been in the ratio of their surfaces, it should have fused nearly fourteen inches.

The absolute effect of a Voltaic apparatus, therefore, seems to be in the compound ratio of the number, and size of the plates: the intensity of the electricity being as the former, the quantity given out as the latter; consequently regard must be had, in its construction, to the purposes for which it is designed. For experiments on perfect conductors, very large plates are to be preferred, a small number of which will probably be sufficient; but where the resistance of imperfect conductors is to be overcome, the combination must be great, but the size of the plates may be small; but if quantity and intensity be both required, then a large number of large plates will be necessary. For general purposes, four inches square will be found to be the most convenient size.

Of the two methods usually employed, that of having the copper and zinc plates joined together only in one point, and moveable, is much better than the old plan of soldering them together, through the whole surface, and cementing them into the troughs: as, by the new construction, the apparatus can be more easily cleaned and repaired, and a double quantity of surface is obtained. For the partitions in the troughs,

glass seems the substance best adapted to secure a perfect insulation ; but the best of all, will be troughs made entirely of WEDGWOOD's ware, an idea, I believe, first suggested by Dr. BABINGTON.

III. *The Bakerian Lecture. An Account of some new analytical Researches on the Nature of certain Bodies, particularly the Alkalies, Phosphorus, Sulphur, Carbonaceous Matter, and the Acids hitherto undecompounded; with some general Observations on Chemical Theory.* By Humphry Davy, *Esq. Sec. R. S. F. R. S. Ed. and M. R. I. A.*

Read December 15, 1808.

1. *Introduction.*

I<small>N</small> the following pages, I shall do myself the honour of laying before the Royal Society, an account of the results of the different experiments, made with the hopes of extending our knowledge of the principles of bodies by the new powers and methods arising from the applications of electricity to chemistry, some of which have been long in progress, and others of which have been instituted since their last session.

The objects which have principally occupied my attention, are the elementary matter of ammonia, the nature of phosphorus, sulphur, charcoal, and the diamond, and the constituents of the boracic, fluoric, and muriatic acids.

Amongst the numerous processes of decomposition, which I have attempted, many have been successful; and from those which have failed, some new phenomena have usually resulted which may possibly serve as guides in future inquiries. On this account, I shall keep back no part of the investigation, and I shall trust to the candour of the Society for an excuse for its imperfection.

The more approaches are made in chemical inquiries towards the refined analysis of bodies, the greater are the obstacles which present themselves, and the less perfect the results.

All the difficulties which occur in analysing a body, are direct proofs of the energy of attraction of its constituent parts. In the play of affinities with respect to secondary compounds even, it rarely occurs that any perfectly pure or unmixed substance is obtained; and the principle applies still more strongly to primary combinations.

The first methods of experimenting on new objects likewise are necessarily imperfect; novel instruments are demanded, the use of which is only gradually acquired, and a number of experiments of the same kind must be made, before one is obtained from which correct data for conclusions can be drawn.

2. *Experiments on the Action of Potassium on Ammonia, and Observations on the Nature of these two Bodies.*

In the Bakerian lecture, which I had the honour of reading before the Society, November 19, 1807, I mentioned that in heating potassium strongly in ammonia, I found that there was a considerable increase of volume of the gas, that hydrogene and nitrogene were produced, and that the potassium appeared to be oxidated; but this experiment, as I had not been able to examine the residuum with accuracy, I did not publish. I stated it as an evidence, which I intended to pursue more fully, of the existence of oxygene in ammonia.

In a paper read before the Royal Society last June, which they have done me the honour of printing, I have given an

account of various experiments on the amalgam from ammonia, discovered by Messrs. BERZELIUS and PONTIN, and in a note attached to this communication, I ventured to controvert an opinion of M. M. GAY LUSSAC and THENARD, with respect to the agency of potassium and ammonia, even on their own statement of facts, as detailed in the Moniteur for May 27, 1808.

The general obscurity belonging to these refined objects of research, their importance and connection with the whole of chemical theory, have induced me since that time to apply to them no inconsiderable degree of labour and attention; and the results of my inquiries will, I trust, be found not only to confirm my former conclusions; but likewise to offer some novel views.

In the first of these series of operations on the action of potassium on ammonia, I used retorts of green glass; I then suspecting oxygene might be derived from the metallic oxides in the green glass, employed retorts of plate glass, and last of all, I fastened the potassium upon trays of platina, or iron, which were introduced into the glass retorts furnished with stop cocks. These retorts were exhausted by an excellent air pump, they were filled with hydrogene, exhausted a second time, and then filled with ammonia from an appropriate mercurial gas holder.* In this way the gas was operated upon in a high degree of purity, which was always ascertained; and all the operations performed out of the contact of mercury, water, or any substances that could interfere with the results.

I at first employed potassium procured by electricity; but

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* A representation of the instruments and apparatus is annexed.

I soon substituted for it the metal obtained by the action of ignited iron upon potash, in the happy method discovered by M. M. GAY LUSSAC and THENARD, finding that it gave the same results, and could be obtained of an uniform quality,* and in infinitely larger quantities, and with much less labour and expense.

When ammonia is brought in contact with about twice its weight of potassium at common temperatures, the metal loses its lustre and becomes white, there is a slight diminution in the volume of the gas; but no other effects are produced. The white crust examined proves to be potash, and the ammonia is found to contain a small quantity of hydrogene, usually not more than equal in volume to the metal. On heating the potassium in the gas, by means of a spirit lamp applied to the bottom of the retort, the colour of the crust is seen to change from white to a bright azure, and this gradually passes through shades of bright blue and green into dark olive. The crust and the metal then fuse together; there is a considerable effervescence, and the crust passing off to the sides, suffers the brilliant surface of the potassium to appear. When the potassium is cooled in this state it is again covered with the white crust. By heating a second time, it swells considerably, becomes porous, and appears crystallized, and of a beautiful

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* When the potash used for procuring potassium in this operation was very pure, and the iron turnings likewise very pure and clean, and the whole apparatus free from any foreign matters, the metal produced differed very little, in its properties, from that obtained by the VOLTAIC battery. Its lustre, ductility, and inflammability were similar. Its point of fusion and specific gravity were, however, a little higher it requiring nearly 130° of FAHRENHEIT to render it perfectly fluid, and being to water as 7960 to 10000, at 60° FAHRENHEIT. This I am inclined to attribute to its containing a minute proportion of iron.

azure tint; the same series of phenomena, as those before described, occur in a continuation of the process, and it is finally entirely converted into the dark olive coloured substance.

In this operation, as has been stated by M. M. Gay Lussac and Thenard, a gas which gives the same diminution by detonation with oxygene, as hydrogene is evolved, and ammonia disappears.

The proportion of the ammonia which looses its elastic form, as I have found by numerous trials, varies according as the gas employed contains more or less moisture.

Thus eight grains of potassium, during its conversion into the olive coloured substance, in ammonia saturated with water at 63° Fahrenheit, and under a pressure equal to that of 29.8 inches of mercury, had caused the disappearance of twelve cubical inches and a half of ammonia; but the same quantity of metal acted upon under similar circumstances, except that the ammonia had been deprived of as much moisture as possible by exposure for two days to potash that had been ignited, occasioned a disappearance of sixteen cubical inches of the volatile alkali.

Whatever be the degree of moisture of the gas, the quantities of inflammable gas generated have always appeared to me to be equal for equal quantities of metal. M. M. Gay Lussac and Thenard are said to have stated, that the proportions in their experiment were the same as would have resulted from the action of water upon potassium. In my trials, they have been rather less. Thus, in an experiment conducted with every possible attention to accuracy of manipulation, eight grains of potassium generated, by their operation upon water,

eight cubical inches and a half of hydrogene gas: and eight grains from the same mass, by their action upon ammonia, produced eight cubical inches and one eighth of inflammable gas. This difference is inconsiderable, yet I have always found it to exist, even in cases where the ammonia has been in great excess, and every part of the metal apparently converted into the olive coloured substance.

No other account of the experiments of M. M. GAY LUSSAC and THENARD has, I believe, as yet been received in this country, except that in the Moniteur already referred to; and in this no mention is made of the properties of the substance produced by the action of ammonia on potassium. Having examined them minutely and found them curious, I shall generally describe them.

1. It is crystallized and presents irregular facets, which are extremely dark, and in colour and lustre not unlike the protoxide of iron; it is opaque when examined in large masses, but is semi-transparent in thin films, and appears of a bright brown colour by transmitted light.

2. It is fusible at a heat a little above that of boiling water, and if heated much higher, emits globules of gas.

3. It appears to be considerably heavier than water, for it sinks rapidly in oil of sassafras.

4. It is a non-conductor of electricity.

5. When it is melted in oxygene gas, it burns with great vividness, emitting bright sparks. Oxygene is absorbed, nitrogene is emitted, and potash, which from its great fusibility seems to contain water, is formed.

6. When brought in contact with water, it acts upon it with much energy, produces heat, and often inflammation, and

evolves ammonia. When thrown upon water, it disappears with a hissing noise, and globules from it often move in a state of ignition upon the surface of the water. It rapidly effervesces and deliquesces in air, but can be preserved under naphtha, in which, however, it softens slowly, and seems partially to dissolve. When it is plunged under water filling an inverted jar, by means of a proper tube, it disappears instantly with effervescence, and the non-absorbable elastic fluid liberated is found to be hydrogene gas.

By far the greatest part of the ponderable matter of the ammonia, that disappears in the experiment of its action upon potassium, evidently exists in the dark fusible product. On weighing a tray containing six grains of potassium, before and after the process, the volatile alkali employed having been very dry, I found that it had increased more than two grains ; the rapidity with which the product acts upon moisture, prevented me from determining the point with great minuteness ; but I doubt not, that the weight of the olive coloured substance and of the hydrogene disengaged precisely equals the weight of the potassium, and ammonia consumed.

. M.M. Gay Lussac and Thenard* are said to have procured from the fusible substance, by the application of a strong h.at, two fifths of the quantity of ammonia that had disappeared in their first process, and a quantity of hydrogene and nitrogene in the proportions in which they exist in ammonia, equal to one fifth more.

* No notice is taken of the apparatus used by M. M. Gay Lussac and Thenard in the Moniteur; but, from the tenour of the details, it seems that they must have operated in glass vessels in the way heretofore adopted over mercury.

My results have been very different, and the reasons will, I trust, be immediately obvious.

When the retort containing the fusible substance is exhausted, filled with hydrogene and exhausted a second time, and heat gradually applied, the substance soon fuses, effervesces, and, as the heat increases, gives off a considerable quantity of elastic fluid, and becomes at length, when the temperature approaches nearly to dull redness, a dark gray solid, which, by a continuance of this degree of heat, does not undergo any alteration.

In an experiment, in which eight grains of potassium had absorbed sixteen cubical inches of well dried ammonia in a glass retort, the fusible substance gave off twelve cubical inches and half of gas, by being heated nearly to redness, and this gas analysed, was found to consist of three quarters of a cubical inch of ammonia, and the remainder of elastic fluids, which when mixed with oxygene gas in the proportion of $6\frac{1}{2}$ to 6, and acted upon by the electric spark diminished to $5\frac{1}{2}$. The temperature of the atmosphere, in this process, was $57°$ Fahrenheit, and the pressure equalled that of 30.1 inches of mercury.

In a similar experiment, in which the platina tray containing the fusible substance was heated in a polished iron tube, filled with hydrogene gas, and connected with a pneumatic apparatus containing very dry mercury, the quantity of elastic fluid given off all the corrections being made, equalled thirteen cubical inches and three quarters, and of these a cubical inch was ammonia; and the residual gas, and the gas introduced into the tube being accounted for, it appeared that the elastic fluid generated, destructible by detonation with

oxygene, was to the indestructible elastic fluid, as 2.5 to 1.

In this process, the heat applied approached to the dull red heat. The mercury, in the thermometer, stood at 62° FAHREN-HEIT, and that in the barometer at 30.3 inches.

In various experiments on different quantities of the fusible substance, in some of which the heat was applied to the tray in the green glass retort, and in others, after it had been introduced into the iron tube; and in which the temperature was sometimes raised slowly and sometimes quickly, the comparative results were so near these that I have detailed, as to render any statement of them superfluous.

A little more ammonia, and rather a larger proportion of inflammable gas,* were in all instances evolved when the iron tube was used, which I am inclined to attribute to the following circumstances. When the tray was brought through the atmosphere to be introduced into the iron tube, the fusible substance absorbed a small quantity of moisture from the air, which is connected with the production of ammonia. And in the process of heating in the retort, the green glass was blackened, and I found that it contained a very small quantity of the oxides of lead and iron, which must have caused the disappearance of a small quantity of hydrogene.

M. M. GAY LUSSAC and THENARD, it appears from the statement, had brought the fusible substance in contact with mercury, which must have given to it some moisture; and when ever this is the case, it furnishes by heat variable quantities

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* The average of six experiments made in a tube of iron, is 2.4 of inflammable gas, to 1 of uninflammable. The average of three made in green glass retorts, is 2.3 to 1.

of ammonia. In one instance, in which I heated the fusible substance from nine grains of potassium, in a retort that had been filled with mercury in its common state of dryness, I obtained seven cubical inches of ammonia as the first product; and in another experiment which had been made with eight grains, and in which moisture was purposely introduced, I obtained nearly nine cubical inches of ammonia, and only four of the mixed gases.

I am inclined to believe, that if moisture could be introduced only in the proper proportion, the quantity of ammonia generated, would be exactly equal to that which disappeared in the first process.

This idea is confirmed by the trials which I have made, by heating the fusible substance with potash, containing its water of crystallization, and muriate of lime partially dried.*

In both these cases, ammonia was generated with great rapidity, and no other gas, but a minute quantity of inflammable gas, evolved, which was condensed by detonation with oxygene with the same phenomena as pure hydrogene.

In one instance, in which thirteen cubical inches of ammonia had disappeared, I obtained nearly eleven and three quarters by the agency of the water of the potash; the quantity of inflammable gas generated, was less than four tenths of a cubical inch.

In another, in which fourteen cubical inches had been

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* If water, in its common form, is brought in contact with the fusible substance, it is impossible to regulate the quantity, so as to gain conclusive results, and a very slight *excess* of water causes the disappearance of a very large quantity of the ammonia generated. In potash and muriate of lime, in certain states of dryness, the water is too strongly attracted by the saline matter to be given off, except for the purpose of *generating* the ammonia.

absorbed, I procured by the operation of the moisture of muriate of lime, nearly eleven cubical inches of volatile alkali, and half a cubical inch of inflammable gas; and the differences, there is every reason to believe, were owing to an excess of water in the salts, by which some of the gas was absorbed.

Whenever, in experiments on the fusible substance, it has been procured from ammonia saturated with moisture, I have always found that more ammonia is generated from it by mere heat; and the general tenour of the experiments incline me to believe, that the small quantity, produced in experiments performed in vacuo, is owing to the small quantity of moisture furnished by the hydrogene gas introduced, and that the fusible substance, heated out of the presence of moisture, is incapable of producing volatile alkali.

M. M. Gay Lussac and Thenard, it is stated, after having obtained three fifths of the ammonia or its elements that had disappeared in their experiment, by heating the product; procured the remaining two fifths, by adding water to the residuum, which after this operation was found to be potash. No notice is taken of the properties of this residuum, which as the details seem to relate to a single experiment, probably was not examined; nor as moisture was present at the beginning of their operations could any accurate knowledge of its nature have been gained.

I have made the residuum of the fusible substance after it has been exposed to a dull red heat, out of the contact of moisture, an object of particular study, and I shall detail its general properties.

It was examined under naphtha, as it is instantly destroyed by the contact of air.

1. Its colour is black, and its lustre not much inferior to that of plumbago.

2. It is opaque even in the thinnest films.

3. It is very brittle, and affords a deep gray powder.

4. It is a conductor of electricity.

5. It does not fuse at a low red heat, and when raised to this temperature, in contact with plate glass, it blackens the glass, and a grayish sublimate rises from it, which likewise blackens the glass.

6. When exposed to air at common temperatures, it usually takes fire immediately, and burns with a deep red light.

7. When it is acted upon by water, it heats, effervesces most violently, and evolves volatile alkali, leaving behind nothing but potash. When the process is conducted under water, a little inflammable gas is found to be generated. A residuum of eight grains giving in all cases about $\frac{20}{100}$ of a cubical inch.

8. It has no action upon quicksilver.

9. It combines with sulphur and phosphorus by heat, without any vividness of effect, and the compounds are highly inflammable, and emit ammonia, and the one phosphuretted and the other sulphuretted hydrogene gas, by the action of water.

As an inflammable gas alone, having the obvious properties of hydrogene is given off during the action of potassium upon ammonia, and as nothing but gases apparently the same as hydrogene and nitrogene, nearly in the proportions in which they exist in volatile alkali, are evolved during the exposure of the compound to the degree of heat which I have specified; and as the residual substance produces ammonia with a little hydrogene by the action of water, it occurred to

me, that, on the principles of the antiphlogistic theory, it ought to be a compound of potassium, a little oxygene and nitrogene, or a combination of a suboxide of potassium and nitrogene; for the hydrogene disengaged in the operations of which it was the result, nearly equalled the whole quantity contained in the ammonia employed; and it was easy to explain the fact of the reproduction of the ammonia by water, on the supposition, that by combination with one portion of the oxygene of the water, the oxide of potassium became potash, and by combination with another portion and its hydrogene, the nitrogene was converted into volatile alkali.

With a view to ascertain this point, I made several experiments on various residuums, procured in the way that I have just stated, from the action of equal quantities of potassium on dry ammonia in platina trays, each portion of metal equalling six grains.

In the first trials, I endeavoured to ascertain the quantity of ammonia generated by the action of water upon a residuum, by heating it with muriate of lime or potash partially deprived of moisture; and after several trials, many of which failed, I succeeded in obtaining four cubical inches and a half of ammonia. In three other cases, where there was reason to suspect a small excess of water, the quantities of ammonia were three cubical inches and a half, three and eight tenths, and four and two tenths.

These experiments were performed in the iron tube used for the former process; the tray was not withdrawn; but the salt introduced in powder, and the apparatus exhausted as before, then filled with hydrogene, and then gently heated in a small portable forge.

Having ascertained what quantity of ammonia was given off from the residuum, I endeavoured to discover what quantity of nitrogene it produced in combustion, and what quantity of oxygene it absorbed. The methods that I employed, were by introducing the trays into vessels filled with oxygene gas over mercury. The product often inflamed spontaneously, and could always be made to burn by a slight degree of heat.

In the trial that I regard as the most accurate, two cubical inches and a half of oxygene were absorbed, and only a cubical inch and one tenth of nitrogene evolved.

Surprised at the smallness of the quantity of the nitrogene, I sought for ammonia in the products of these operations; but various trials convinced me that none was formed. I examined the solid substances produced, expecting nitrous acid; but the matter proved to be dry potash, apparently pure, and not affording the slightest traces of acid.

The quantity of nitrogene existing in the ammonia, which this residuum would have produeed by the action of *water*, supposing the volatile alkali decomposed by electricity, would have equalled at least two cubical inches and a quarter.

I heated the same proportions of residuum with the red oxide of mercury, and the red oxide of lead in vacuo, expecting that when oxygene was supplied in a gradual way, the result might be different from that of combustion; but in neither of these cases did the quantity of nitrogene exceed a cubical inch and a half.

But on what could this loss of nitrogene depend; had it entered into any unknown form with oxygene, or did it not really exist in the residuum in the same quantity, as in the *ammonia* produced from it?

I hoped that an experiment of exposing the residuum to intense heat might enlighten the inquiry. I distilled one of the portions which had been covered with naphtha, in a tube of wrought platina made for the purpose. The tube had been exhausted and filled with hydrogene, and exhausted again, and was then connected with a pneumatic mercurial apparatus. Heat was at first slowly applied till the naphtha had been driven over. It was then raised rapidly by an excellent forge. When the tube became cherry red, gas was develloped; it continued to be generated for some minutes. When the tube had received the most intense heat that could be applied, the operation was stopped. The quantity of gas collected, making the proper corrections and reductions, would have been three cubical inches and a half at the mean temperature and pressure. Twelve measures of it were mixed with six of oxygene gas, the electrical spark was passed through the mixture; a strong inflammation took place, the diminution was to three measures and a half, and the residuum contained oxygene. This experiment was repeated upon different quantities with the same comparative results.

In examining the platina tube, which had a screw adapted to it at the lower extremity, by means of which it could be opened. The lower part was found to contain potash, which had all the properties of the pure alkali, and in the upper part there was a quantity of potassium. Water poured into the tube, produced a violent heat and inflammation; but no smell of ammonia.

This result was so unexpected and so extraordinary, that I at first supposed there was some source of error. I had calculated upon procuring nitrogene as the only aeriform

product; I obtained an elastic fluid which gave much more diminution by detonation with oxygene, than that produced from ammonia by electricity.

I now made the experiment, by heating the entire fusible substance, from six grains of potassium which had absorbed twelve cubical inches of ammonia, in the iron tube, in the manner before described. The heat was gradually raised to whiteness, and the gas collected in two portions. The whole quantity generated, making the usual corrections for temperature and pressure, and the portion of hydrogene originally in the tube, and the residuum, would have been fourteen cubical inches and a half at the mean degree of the barometer and thermometer. Of these, nearly a cubical inch was ammonia and the remainder a gas, of which the portion destructible by detonation with oxygene, was to the indestructible portion, as 2.7 to 1.

The lower part of the tube, where the heat had been intense, was found surrounded with potash in a vitreous form; the upper part contained a considerable quantity of potassium.

In another similar experiment, made expressly for the purposes of ascertaining the quantity of potassium recovered, the same elastic products were evolved. The tube was suffered to cool, the stop-cock being open in contact with mercury, it was filled with mercury, and the mercury displaced by water; when two cubical inches and three quarters of hydrogene gas were generated, which proved that at least two grains and a half of potassium had been revived.

Now, if a calculation be made upon the products in these operations, considering them as nitrogene and hydrogene, and taking the common standard of temperature and pressure, it

will be found, that by the decomposition of 11 cubical inches of ammonia equal to 2.05 grains, there is generated 3.6 cubical inches of nitrogene equal to 1.06 grains, and 9.9 cubical inches of hydrogene, which added to that disengaged in the first operation equal to about 6.1 cubical inches, are together equal to .382 grains; and the oxygene added to 3.5 grains of potassium would be .6 grains, and the whole amount is 2.04 grains; and 2.05 — 2.04 = .01. But the same quantity of ammonia, decomposed by electricity, would have given 5.5 cubical inches of nitrogene equal to 1.6 grains, and only 14 cubical inches of hydrogene* equal to .33, and allowing the separation of oxygene in this process in water, it cannot be estimated at more than .11 or .12.

So that if the analysis of ammonia by electricity at all approaches towards accuracy; in the process just described, there is a considerable loss of nitrogene, and a production of oxygene and inflammable gas.

And in the action of water upon the residuum, in the experiment page 52, there is an apparent generation of nitrogene.

How can these extraordinary results be explained?

The decomposition and composition of nitrogene seem proved, allowing the correctness of the data; and one of its elements appears to be oxygene; but what is its other elementary matter?

Is the gas that appears to possess the properties of hydrogene, a new species of inflammable aeriform substance?

Or has nitrogene a metallic basis which alloys with the iron or platina?

* See Phil. Trans. 1808, p. 40.

Or is water alike the *ponderable* matter of nitrogene, hydrogene, and oxygene?

Or is nitrogene a compound of hydrogene with a larger proportion of oxygene than exists in water?

These important questions, the two first of which seem the least likely to be answered in the affirmative, from the correspondence between the weight of the ammonia decomposed, and the products, supposing them to be known substances, I I shall use every effort to solve by new labours, and I hope soon to be able to communicate the results of further experiments on the subject to the Society.

As the inquiry now stands, it is however sufficiently demonstrative, that the opinion which I had ventured to form respecting the decomposition of ammonia in this experiment, is correct; and that M. M. GAY LUSSAC's and THENARD's idea of the decomposition of the potassium, and their theory of its being compounded of hydrogene and potash, are unfounded.

For a considerable part of the potassium is recovered unaltered, and in the entire decomposition of the fusible substance, there is only a small excess of hydrogene above that existing in the ammonia acted upon.

The mere phenomena of the process likewise, if minutely examined, prove the same thing.

After the first slight effervescence, owing to the water absorbed by the potash formed upon the potassium during its exposure to the air, the operation proceeds with the greatest tranquillity. No elastic fluid is given off from the potassium; it often appears covered with the olive coloured substance, and if it were evolving hydrogene; this must pass through

the fluid; but even to the end of the operation, no such appearance occurs.

The crystallized and spongy substance, formed in the first part of the process, I am inclined to consider as a combination of ammonium and potassium, for it emits a smell of ammonia when exposed to air, and is considerably lighter than potassium.

I at first thought that a solid compound of hydrogene and potassium might be generated in the first part of this operation: but experiments on the immediate action of potassium and hydrogene did not favour this opinion. Potassium, as I ventured to conclude in the Bakerian Lecture for 1807,*is very

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* M. M. GAY LUSSAC and THENARD seem to be of a different opinion. In the Moniteur, to which I have so often referred, it is related, that these distinguished chemists, exposing hydrogene to potassium at a high temperature, found that the hydrogene was absorbed, and that it formed a compound with the potassium of a light gray colour, from which hydrogene was capable of being obtained by the action of water or mercury.

After a number of trials, I have not been able to witness this result. In an experiment which I made in the presence of Mr. PEPYS, and which I have often repeated, and twice before a numerous assembly, in retorts of plate glass, four grains of potassium were heated in fourteen cubical inches of pure hydrogene. At first, white fumes arose and precipitated themselves in the neck of the retort. When a considerable film of the precipitate had collected, its colour appeared a bright gray, and after the first two or three minutes, it ceased to be formed.

The bottom of the retort was heated to redness, when the potassium began to sublime and condense on the sides.

The process was stopped, and the retort suffered to cool. The absorption was not equal to a quarter of a cubical inch. When the retort was *broken*, the gas in passing into the atmosphere, produced an explosion with most vivid light, and white fumes. The potassium remaining in the retort, and that which had sublimed, seemed unaltered in their properties.

The grayish substance inflamed by the action of water, but did not seem to be combinable with mercury. I am inclined to attribute its formation to the agency of

soluble in hydrogene; but, under common circumstances, hydrogene does not seem to be absorbable by potassium.

moisture suspended in the hydrogene, and to consider it as a triple compound of potassium, oxygene, and hydrogene.

When potassium is heated in a gas containing hydrogene, and from $\frac{1}{15}$ to $\frac{1}{30}$ of common air, it is formed in greater quantities, and a crust of it covers the metal, and in the process there is an absorption both of hydrogene and oxygene. It is likewise produced in experiments on the generation of potassium by exposing potash to ignited iron, at the time (I believe) that common air is admitted, during the cooling of the tube.

It is non-conducting, inflames spontaneously in air, and produces potash and aqueous vapour by its combustion.

When potassium is heated in hydrogene in a flint glass retort, or even for a great length of time in a green glass retort, there is an absorption of the gas; but this is independent of the presence of potassium, and is owing to the action of the metallic oxides in the glass upon the hydrogene.

If a solid compound of hydrogene and potassium could be formed, we might expect its existence in the experiment with the gun barrel, in which potassium is exposed to hydrogene at almost every temperature; but the metal formed in this process, when proper precautions are taken to exclude carbonaceous matters, is uniform in its properties, and generates for equal quantities, equal proportions of hydrogene by the action of water.

The general phenomena of this operation, shew indeed that the solution of potassium in hydrogene is intimately connected with the general principle of the decomposition, and confirm my first idea of the action of the two bodies.

Hydrogene dissolves a large quantity of potassium by heat, but the greater portion is precipitated on cooling. The attractions which determine the chemical change, seem to be that of iron for oxygene, of iron for potassium, and of hydrogene for potassium; and in experiments, in which a very intense heat is used for the production of potassium by iron, I have often found, that the gas which comes over, though it has passed through a tube cooled by ice, inflames spontaneously in the atmosphere, and burns with a most brilliant light which is purple at the edges, and throws off a dense vapour containing potash.

Sodium appears to be almost insoluble in Hydrogene, and this seems to be one reason why it cannot be obtained, except in very minute quantities, in the experiment with the gun barrel,

Still tin, though scarcely capable of being dissolved in hydrogene alone, seems to be

3. *Analytical Experiments on Sulphur.*

I have referred, on a former occasion,* to the experiments of Mr. CLAYFIELD and of Mr. BERTHOLLET, jun. which seemed to show that sulphur, in its common form, contained hydrogene. In considering the analytical powers of the VOLTAIC apparatus, it occurred to me, that though sulphur, from its being a non-conductor, could not be expected to yield its elements to the electrical attractions and repulsions of the opposite surfaces, yet that the intense heat, connected with the contact of these surfaces, might possibly effect some alteration in it, and tend to separate any elastic matter it might contain.

On this idea some experiments were instituted in 1807. A curved glass tube, having a platina wire hermetically sealed in its upper extremity, was filled with sulphur. The sulphur was melted over a spirit lamp; and a proper connection being made with the VOLTAIC apparatus of one hundred plates of six inches, in great activity, a contact was made in the sulphur by means of another platina wire. A most brilliant spark, which appeared orange coloured through the sulphur, was produced, and a minute portion of elastic fluid rose to the upper extremity of the tube. By a continuation of the

soluble in the compound of hydrogene and potassium. By exposing mixtures of potash and soda, to ignited iron I have obtained some very curious alloys; which, whether the potassium or the sodium was in excess, were fluid at common temperatures. The compound containing an excess of potassium was even lighter than potassium (probably from its fluidity). All these alloys were in the highest degree inflammable. When a globule of the fluid alloy was touched by a globule of mercury, they combined with a heat that singed the paper upon which the experiment was made, and formed, when cool, a solid so hard, as not to be cut by a knife.

* Bakerian Lecture, 1808, p. 16.

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process for nearly an hour, a globule equal to about the tenth
of an inch in diameter was obtained, which, when examined,
was found to be sulphuretted hydrogene.

This result perfectly coincided with those which have been
just mentioned ; but as the sulphur that I had used was merely
in its common state, and as the ingenious experiments of Dr.
Thomson have shewn that sulphur in certain forms may con-
tain water, I did not venture, at that time, to form any con-
clusion upon the subject.

In the summer of the present year, I repeated the experi-
ment with every precaution. The sulphur that I employed
was Sicilian sulphur, that had been recently sublimed in a re-
tort filled with nitrogene gas, and that had been kept hot till
the moment that it was used. The power applied was that of
the battery of five hundred double plates of six inches, highly
charged. In this case the action was most intense, the
heat strong, and the light extremely brilliant ; the sulphur
soon entered into ebullition, elastic matter was formed in
great quantities, much of which was permanent ; and the
sulphur, from being of a pure yellow, became of a deep red
brown tint.

The gas, as in the former instance, proved to be sulphu-
retted hydrogene. The platina wires were considerably
acted upon ; the sulphur, at its point of contact with them,
had obtained the power of reddening moistened litmus
paper.

I endeavoured to ascertain the quantity of sulphuretted hy-
drogene evolved in this way from a given quantity of sulphur,
and for this purpose, I electrized a quantity equal to about
two hundred grains in an apparatus of the kind I have just

described, and when the upper part of the tube was full of gas, I suffered it to pass into the atmosphere; so as to enable me to repeat the process.

When I operated in this way, there seemed to be no limit to the generation of elastic fluid, and in about two hours a quantity had been evolved, which amounted to more than five times the volume of the sulphur employed. From the circumstances of the experiment, the last portion only could be examined, and this proved to be sulphuretted hydrogene. Towards the end of the process, the sulphur became extremely difficult of fusion, and almost 'opaque, and when cooled and broken, was found of a dirty brown colour.

The experiments upon the union of sulphur and potassium, which I laid before the Society last year, prove that these bodies act upon each other with great energy, and that sulphuretted hydrogene is evolved in the process, with intense heat and light.

In heating potassium in contact with compound inflammable substances, such as resin, wax, camphor, and fixed oils in close vessels out of the contact of the air, I found that a violent inflammation was occasioned, that hydrocarbonate was evolved; and that when the compound was not in great excess, a substance was formed, spontaneously inflammable at common temperatures, the combustible materials of which were charcoal and potassium.

Here was a strong analogy between the action of these bodies, and sulphur on potassium. Their physical properties likewise resemble those of sulphur; for they agree in being non-conductors, whether fluid or solid, in being transparent when fluid, and semi-transparent when solid, and highly

refractive ; their affections by electricity are likewise similar to those of sulphur ; for the oily bodies give out hydrocarbonate by the agency of the VOLTAIC spark, and become brown, as if from the deposition of carbonaceous matter.

But the resinous and oily substances are compounds of a small quantity of hydrogene and oxygene, with a large quantity of a carbonaceous basis. The existence of hydrogene in sulphur is fully proved, and we have no right to consider a substance, which can be produced from it in such large quantities, merely as an accidental ingredient.

The oily substances in combustion, produce two or three times their weight of carbonic acid and some water ; I endeavoured to ascertain whether water was formed in the combustion of sulphur in oxygene gas, dried by exposure to potash : but in this case sulphureous acid is produced in much larger quantities than sulphuric acid, and this last product is condensed with great difficulty. In cases, however, in which I have obtained, by applying artificial cold, a deposition of acid in the form of a film of dew in glass retorts out of the contact of the atmosphere, in which sulphur had been burned in oxygene gas hygrometrically dry, it has appeared to me less tenacious and lighter than the common sulphuric acid of commerce, which in the most concentrated form in which I have seen it, namely, at 1.855, gave abundance of hydrogene as well as sulphur, at the negative surface in the VOLTAIC circuit, and hence evidently contained water.

The reddening of the litmus paper, by sulphur that had been acted on by VOLTAIC electricity, might be ascribed to its containing some of the sulphuretted hydrogene formed in the process ; but even the production of this gas, as will be im-

mediately seen, is an evidence of the existence of oxygene in sulphur.

In my early experiments on potassium, procured by electricity, I heated small globules of potassium in large quantities of sulphuretted hydrogene, and I found that sulphuret of potash was formed; but this might be owing to the water dissolved in the gas, and I ventured to draw no conclusion till I had tried the experiment in an unobjectionable manner.

I heated four grains of potassium in a retort of the capacity of twenty cubical inches; it had been filled after the usual processes of exhaustion with sulphuretted hydrogene, dried by means of muriate of lime that had been heated to whiteness; as soon as the potassium fused, white fumes were copiously emitted, and the potassium soon took fire, and burnt with a most brilliant flame, yellow in the centre and red towards the circumference.*

The diminution of the volume of the elastic matter, in this operation, did not equal more than two cubical inches and a half. A very small quantity of the residual gas only was absorbable by water. The non-absorbable gas was hydrogene, holding a minute quantity of sulphur in solution.

A yellow sublimate lined the upper part of the retort, which proved to be sulphur. The solid matter formed was red at the surface like sulphuret of potash, but in the interior it was dark gray, like sulphuret of potassium. The piece of the retort containing it was introduced into a jar inverted over mercury,

* In the Moniteur, May 27, 1808, in the account of M. M. GAY LUSSAC's and THENARD's experiments, it is mentioned, that potassium absorbs the sulphur and a part of the hydrogene of sulphuretted hydrogene; but the phenomena of inflammation is not mentioned, nor are the results described.

and acted upon by a small quantity of dense muriatic acid, diluted with an equal weight of water, when there were disengaged two cubical inches and a quarter of gas, which proved to be sulphuretted hydrogene.

In another experiment, in which eight grains of potassium were heated in a retort of the capacity of twenty cubical inches, containing about nineteen cubical inches of sulphuretted hydrogene, and a cubical inch of phosphuretted hydrogene, which was introduced for the purpose of absorbing the oxygene of the small quantity of common air admitted by the stop-cock, the inflammation took place as before, there was a similar precipitation of sulphur on the sides of the retort; the mass formed in the place of the potassium was orange externally, and of a dark gray colour internally, as in the last instance; and when acted on by a little water holding muriatic acid in solution, there were evolved from it five cubical inches only of sulphuretted hydrogene.

Both these experiments concur in proving the existence of a principle in sulphuretted hydrogene, capable of destroying partially the inflammability of potassium, and of producing upon it all the effects of oxygene; for had the potassium combined merely with pure combustible matter, it ought, as will be seen distinctly from what follows, to have evolved by the action of the acid, a volume of sulphuretted hydrogene, at least equal to that of the hydrogene, which an equal weight of uncombined potassium would have produced by its operation upon water.

Sulphuretted hydrogene, as has been long known to chemists, may be formed by heating sulphur strongly in hydrogene gas. I heated four grains of sulphur in a glass retort,

containing about twenty cubical inches of hydrogene, by means of a spirit lamp, and pushed the heat nearly to redness. There was no perceptible change of volume in the gas after the process; the sulphur that had sublimed was unaltered in its properties, and about three cubical inches of an elastic fluid absorbable by water were formed: the solution reddened litmus, and had all the properties of a solution of pure sulphuretted hydrogene. Now if we suppose sulphuretted hydrogene to be constituted by sulphur dissolved in its unaltered state in hydrogene, and allow the existence of oxygene in this gas; its existence must likewise be allowed in sulphur, for we have no right to assume that sulphur in sulphuretted hydrogene is combined with more oxygene than in its common form: it is well known, that when electrical sparks are passed through sulphuretted hydrogene, a considerable portion of sulphur is separated without any alteration in the volume of the gas. This experiment I have made more than once, and I found that the sulphur obtained, in fusibility, combustibility, and other sensible properties did not perceptibly differ from common sublimed sulphur.

According to these ideas, the intense ignition produced by the action of sulphur, on potassium and sodium, must not be ascribed merely to the affinity of the metals of the alkalies for its basis, but may be attributed likewise to the agency of the oxygene that it contains.

The minute examination of the circumstances of the action of potassium and sulphur likewise confirms these opinions.

When two grains of potassium and one of sulphur were heated gently in a green glass tube filled with hydrogene, and connected with a pneumatic apparatus, there was a most

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intense ignition produced by the action of the two bodies, and one eighth of a cubical inch of gas was disengaged, which was sulphuretted hydrogene. The compound was exposed in a mercurial apparatus to the action of liquid muriatic acid; when a cubical inch and quarter of aeriform matter was produced, which proved to be pure sulphuretted hydrogene.

The same experiment was repeated, except that four grains of sulphur were employed instead of one. In this case, a quarter of a cubical inch of gas was disengaged during the process of combination; and when the compound was acted upon by muriatic acid, only three quarters of a cubical inch of sulphuretted hydrogene was obtained.

Now, *sulphuret* of potash produces sulphuretted hydrogene by the action of an acid; and if the sulphur had not contained oxygene, the hydrogene evolved by the action of the potassium in both these experiments ought to have equalled at least two cubical inches, and the whole quantity of sulphuretted hydrogene ought to have been more: and that so much less sulphuretted hydrogene was evolved in the second experiment, can only be ascribed to the larger quantity of oxygene furnished to the potassium by the larger quantity of the sulphur.

I have made several experiments of this kind with similar results. Whenever equal quantities of potassium were combined with unequal quantities of sulphur, and exposed afterwards to the action of muriatic acid, the largest quantity of sulphuretted hydrogene was furnished by the product containing the smallest proportion of sulphur, and in no case was the quantity of gas equal in volume to the quantity of hydrogene, which would have been produced by the mere action of potassium upon water.

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From the general tenour of these various facts, it will not be, I trust, unreasonable to assume, that sulphur, in its common state, is a compound of small quantities of oxygene and hydrogene with a large quantity of a basis that produces the acids of sulphur in combustion, and which, on account of its strong attractions for other bodies, it will probably be very difficult to obtain in its pure form.

In metallic combinations even, it still probably retains its oxygene and part of its hydrogene. Metallic sulphurets can only be partially decomposed by heat, and the small quantity of sulphur evolved from them in this case when perfectly dry and out of the contact of air, as I found in an experiment on the sulphurets of copper and iron, exists in its common state, and acts upon potassium, and is affected by electricity in the same manner as native sulphur.

4. *Analytical Experiments on Phosphorus.*

The same analogies apply to phosphorus as to sulphur, and I have made a similar series of experiments on this inflammable substance.

Common electrical sparks, passed through phosphorus, did not evolve from it any permanent gas ; but when it was acted upon by the VOLTAIC electricity of the battery of five hundred plates in the same manner as sulphur, gas was produced in considerable quantities, and the phosphorus became of a deep red brown colour, like phosphorus that has been inflamed and extinguished under water. The gas examined proved to be phosphuretted hydrogene, and in one experiment, continued for some hours, a quantity estimated to be nearly equal to four times the volume of the phosphorus employed was given

off. The light of the VOLTAIC spark in the phosphorus was at first a brilliant yellow, but as the colour of the phosphorus changed, it appeared orange.

I heated three grains of potassium in sixteen cubical inches of phosphuretted hydrogene; as soon as it was fused, the retort became filled with white fumes, and a reddish substance precipitated upon the sides and upper part of it. The heat was applied for some minutes. No inflammation took place.* When the retort was cool, the absorption was found to be less than a cubical inch. The potassium externally was of a deep brown colour, internally it was of a dull lead colour. The residual gas had lost its property of spontaneous inflammation, but seemed still to contain a small quantity of phosphorus in solution.

The phosphuret acted upon over mercury by solution of muriatic acid evolved only one cubical inch and three quarters of phosphuretted hydrogene.

From this experiment, there is great reason to suppose that phosphuretted hydrogene contains a minute proportion of oxygene, and consequently that phosphorus likewise may contain it; but the action of potassium on phosphorus itself furnishes perhaps more direct evidences of the circumstance.

One grain of potassium and one grain of phosphorus were fused together in a proper apparatus. They combined with the production of the most vivid light and intense ignition. During the process one tenth of a cubical inch of phosphuretted hydrogene was evolved. The phosphuret formed, exposed

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* It is stated, in the account before referred to of M. M. GAY LUSSAC's and THENARD's experiments, that potassium inflames in phosphuretted hydrogene. My experiments upon this gas have been often repeated. I have never perceived any luminous appearance; but I have always operated in day-light.

.to the action of diluted muriatic acid over mercury, produced exactly three tenths of a cubical inch of phosphuretted hydrogene.

In a second experiment, one grain of potassium was fused with three grains of phosphorus; in this case nearly a quarter of a cubical inch of phosphuretted hydrogene was generated during the ignition. But from the compound exposed to muriatic acid, only one tenth of a cubical inch could be procured.

Now it is not easy to refer the deficiency of phosphuretted hydrogene in the second case to any other cause than to the supply of oxygene to the potassium from the phosphorus; and the quantity of phosphuretted hydrogene evolved in the first case, is much less than could be expected, if both potassium and phosphorus consisted merely of pure combustible matter.

The phosphoric acid, formed by the combustion of phosphorus, though a crystalline solid, may still contain water. The hydrogene evolved from phosphorus by electricity proves indeed that this must be the case; and though the quantity of hydrogene and oxygene in phosphorus may be exceedingly small, yet they may be sufficient to give it peculiar characters; and till the basis is obtained free, we shall have no knowledge of the properties of the pure phosphoric element.

5. *On the States of the carbonaceous Principle in Plumbago, Charcoal, and the Diamond.*

The accurate researches of Messrs. ALLEN and PEPYS have distinctly proved, that plumbago, charcoal, and the diamond

produce very nearly the same quantities of carbonic acid, and absorb very nearly the same quantities of oxygene in combustion.

Hence it is evident, that they must consist principally of the same kind of elementary matter ; but minute researches upon their chemical relations, when examined by new analytical methods will, I am inclined to believe, shew that the great difference in their physical properties does not merely depend upon the differences of the mechanical arrangement of their parts, but likewise upon differences in their intimate chemical nature.

I endeavoured to discover, whether any elastic matter could be obtained from plumbago very intensely ignited by the VOLTAIC battery in a Torricellian vacuum : but though the highest power of the battery of five hundred was employed, and though the heat was such, as in another experiment instantly melted platina wire of $\frac{1}{60}$th of an inch in diameter, yet no appearance of change took place upon the plumbago. Its characters remained wholly unaltered, and no permanent elastic fluid was formed.

I heated one grain of plumbago, with twice its weight of potassium, in a plate glass tube connected with a proper apparatus, and I heated an equal quantity of potassium alone in a tube of the same kind, for an equal length of time, namely, eight minutes. Both tubes were filled with hydrogene : no gas was evolved in either case. There was no ignition in the tube containing the plumbago, but it seemed gradually to combine with the potassium. The two results were exposed to the action of water ; the result from the plumbago acted upon that fluid with as much energy as the other result, and

the two volumes of elastic fluids were 1.8 cubical inches and 1.9 cubical inches; and both gave the same diminution by detonation with oxygene, as pure hydrogene. Two grains of potassium, by acting upon water, would have produced two cubical inches and one eighth of hydrogene gas; the deficiency in the result, in which potassium alone was used, must be ascribed to the loss of a small quantity of metal, which must have been carried off in solution in the hydrogene, and perhaps, likewise, to the action of the minute quantity of metallic oxides in the plate glass. The difference in the quantity of hydrogene given off in the two results, is however too slight to ascribe it to the existence of oxygene in the plumbago.

I repeated this experiment several times with like results, and in two or three instances examined the compound formed. It was infusible at a red heat, had the lustre of plumbago. It inflamed spontaneously, when exposed to air, generated potash, and left a black powdery residuum. It effervesced most violently in water, and produced a gas, which burnt like pure hydrogene.

When small pieces of charcoal from the willow, that had been intensely ignited, were acted upon by Voltaic electricity in a Torricellian vacuum, every precaution being taken to exclude moisture from the mercury and the charcoal, the results were very different from those occuring in the case of plumbago.

When plumbago was used, after the first spark, which generally passed through a distance of about one eighth of an inch, there was no continuation of light, without a contact or an approach to the same distance; but from the charcoal a

flame seemed to issue of a most brilliant purple, and formed, as it were, a conducting chain of light of nearly an inch in length, at the same time that elastic matter was rapidly form- ed, some of which was permanent. After many unsuccessful trials, I at length succeeded in collecting the quantity of elastic fluid given out by half a grain of charcoal; the process had been continued nearly half an hour. The quantity of gas amounted to nearly an eighth of a cubical inch; it was inflamma- ble by the electric spark with oxygene gas, and four measures of it absorbed three measures of oxygene, and produced one measure and a half of carbonic acid. The charcoal in this ex- periment had become harder at the point, and its lustre, where it had been heated to whiteness, approached to that of plum- bago.

I heated two grains of potassium together with two grains of charcoal, for five minutes; and to estimate the effects of the metallic oxides and potash in the green glass tube, I made a comparative experiment, as in the case of plumbago; but there was no proof of any oxygene being furnished to the potassium from the charcoal in the process, for the compound acted upon water with great energy, and produced a quantity of inflammable gas, only inferior by one twelfth to that pro- duced by the potassium, which had not been combined with charcoal, and which gave the same diminution by detonation with oxygene; and the slight difference may be well as- cribed to the influence of foreign matters in the charcoal. There was no ignition in the process, and no gas was evolved.

The compound produced in other experiments of this kind was examined. It is a conductor of electricity, is of a dense

black, inflames spontaneously, and burns with a deep red light in the atmosphere.*

The non-conducting nature of the diamond and its infusibility, rendered it impossible to act upon it by Voltaic electricity; and the only new agents which seemed to offer any means of decomposing it, were the metals of the alkalies.

When a diamond is heated in a green glass tube with potassium, there is no elastic fluid given out, and no intensity of action; but the diamond soon blackens, and scales seem to detach themselves from it, and these scales, when examined in the magnifier, are gray externally, and of the colour of plumbago internally, as if they consisted of plumbago covered by the gray oxide of potassium.

In heating together three grains of diamonds in powder, and two grains of potassium, for an hour in a small retort of plate glass filled with hydrogene, and making the comparative trial with two grains of potassium heated in a similar apparatus, without any diamonds, I found that the potassium which had been heated with the diamonds, produced, by its action upon water, one cubical inch and $\frac{3}{10}$ of inflammable air, and that which had been exposed to heat alone, all other circumstances being similar, evolved nearly one cubical inch and $\frac{7}{10}$, both of which were pure hydrogene.

In another experiment of a similar kind, in which fragments of diamonds were used in the quantity of four grains, the potassium became extremely black from its action upon them during an exposure to heat for three hours, and the diamonds

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* In the Bakerian Lecture for 1807, I have mentioned the decomposition of carbonic acid by potassium, which takes place with inflammation. If the potassium is in excess in this experiment, the same pyrophorus as that described above is formed.

were covered with a grayish crust, and when acted upon by water and dried, were found to have lost about $\frac{r_2 8}{100}$ of a grain in weight. The matter separated by washing, and examined, appeared as a fine powder of a dense black colour. When a surface of platina wire was covered with it, and made to touch another wire in the VOLTAIC circuit, a brilliant spark with combustion occurred. It burnt, when heated to redness in a green glass tube filled with oxygene gas, and produced carbonic acid by its combustion.

These general results seem to shew, that in plumbago the carbonaceous element exists merely in combination with iron, and in a form which may be regarded as approaching to that of a metal in its nature, being conducting in a high degree, opaque, and possessing considerable lustre.

Charcoal appears to contain a minute quantity of hydrogene in combination. Possibly likewise, the alkalies and earths produced during its combustion, exist in it not fully combined with oxygene, and according to these ideas, it is a very compounded substance, though in the main it consists of the pure carbonaceous element.

The experiments on the diamond render it extremely likely that it contains oxygene; but the quantity must be exceedingly minute, though probably sufficient to render the compound non-conducting: and if the carbonaceous element in charcoal and the diamond be considered as united to still less foreign matter in quantity, than in plumbago, which contains about $\frac{1}{70}$ of iron, the results of their combustion, as examined independently of hygrometrical tests, will not differ perceptibly.

Whoever considers the difference between iron and steel, in which there does not exist more than $\frac{1}{200}$ of plumbago, or the

difference between the amalgam of ammonium, and mercury, in which the quantity of new matter is not more than $\frac{1}{12000}$, or that between the metals and their sub-oxides, some of which contain less than $\frac{1}{10}$ of oxygene, will not be disposed to question the principle, that minute differences in chemical composition may produce great differences in external and physical characters.

6. *Experiments on the Decomposition, and Composition of the Boracic Acid.*

In the last Bakerian Lecture,* I have given an account of an experiment in which boracic acid appeared to be decomposed by VOLTAIC electricity, a dark coloured inflammable substance separating from it on the negative surface.

In the course of the spring and summer, I made many attempts to collect quantities of this substance for minute examination. When boracic acid, moistened with water, was exposed between two surfaces of platina, acted on by the full power of the battery of five hundred, an olive-brown matter immediately began to form on the negative surface, which gradually increased in thickness, and at last appeared almost black. It was permanent in water, but soluble with effervescence in warm nitrous acid. When heated to redness upon the platina it burnt slowly, and gave off white fumes, which slightly reddened moistened litmus paper, and it left a black mass, which, when examined by the magnifier, appeared vitreous at the surface, and evidently contained a fixed acid.

These circumstances seemed distinctly to shew the decomposition, and recomposition of the boracic acid; but as the

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peculiar combustible substance was a non-conductor of elec-
tricity, I was never able to obtain it, except in very thin films
upon the platina. It was not possible to examine its proper-
ties minutely, or to determine its precise nature, or whether
it was the pure boracic basis; I consequently endeavoured to
apply other methods of decomposition, and to find other more
unequivocal evidences upon this important chemical subject.

I have already laid before the Society an account of an ex-
periment,* in which boracic acid, heated in contact with potas-
sium in a gold tube, was converted into borate of potash, at
the same time that a dark coloured matter, similar to that pro-
duced from the acid by electricity, was formed. About two
months after this experiment had been made, namely, in the
beginning of August, at a time that I was repeating the pro-
cess, and examining minutely the results, I was informed, by a
letter from Mr. CADELL at Paris, that M. THENARD was em-
ployed in the decomposition of the boracic acid by potassium,
and that he had heated the two substances together in a copper
tube, and had obtained borate of potash, and a peculiar matter
concerning the nature of which no details were given in the
communication.

That the same results must be obtained by the same me-
thods of operating, there could be no doubt. The evidences
for the decomposition of the boracic acid are easily gained,
the synthetical proofs of its nature involve more complicated
circumstances.

I found that when equal weights of potassium and boracic
acid were heated together in a green glass tube, which had
been exhausted after having been twice filled with hydro-

* Phil. Trans. Part II. 1808. p. 343.

gene, there was a most intense ignition before the temperature was nearly raised to the red heat; the potassium entered into vivid inflammation, where it was in contact with the boracic acid. When this acid had been heated to whiteness, before it was introduced into the tube, and powdered and made use of whilst yet warm, the quantity of gas given out in the operation did not exceed twice the volume of the acid, and was hydrogene.

I could only use twelve or fourteen grains of each of the two substances in this mode of conducting the experiment; for when larger quantities were employed, the glass tube always ran into fusion from the intensity of the heat produced during the action.

When the film of naphtha had not been carefully removed from the potassium, the mass appeared black throughout; but when this had been the case, the colour was of a dark olivebrown.

In several experiments, in which I used equal parts of the acid and metal, I found that there was always a great quantity of the former in the residuum, and by various trials, I ascertained that twenty grains of potassium had their inflammability entirely destroyed by about eight grains of boracic acid.

For collecting considerable portions of the matters formed in the process, I used metallic tubes furnished with stop-cocks, and exhausted after being filled with hydrogene.

When tubes of brass or copper were employed, the heat was only raised to a dull red; but when iron tubes were used, it was pushed to whiteness. In all cases the acid was decomposed, and the products were scarcely different.

When the result was taken out of a tube of brass or copper,

it appeared as an olive coloured glass, having opaque, dull olive-brown specks diffused through it.

It gave a very slight effervescence with water, and partially dissolved in hot water, a dark olive coloured powder separating from it.

The results from the iron tube, which had been much more strongly heated, were dark olive in some parts, and almost black in others. They did not effervesce with warm water, but were rapidly acted upon by it, and the particles separated by washing, were of a shade of olive, so dark as to appear almost black on white paper.

The solutions obtained, when passed through a filter, had a faint olive tint, and contained sub-borate of potash, and potash. In cases, when instead of water, a weak solution of muriatic acid was used for separating the saline matter, from the inflammable matter, the fluid came through the filter colourless.

In describing the properties of the new inflammable substance separated by washing, I shall speak of that collected from operations conducted in tubes of brass, in the manner that has been just mentioned; for it is in this way, that I have collected the largest quantities.

It appears as a pulverulent mass of the darkest shades of olive. It is perfectly opaque. It is very friable, and its powder does not scratch glass. It is a non-conductor of electricity.

When it has been dried only at 100 or 120°, it gives off moisture by increase of temperature, and if heated in the atmosphere, takes fire at a temperature below the boiling point of olive oil, and burns with a red light and scintillations like charcoal.

If it be excluded from air and heated to whiteness in a tube of platina, exhausted after having been filled with hydrogene, it is found very little altered after the process. Its colour is a little darker, and it is rather denser; but no indications are given of any part of it having undergone fusion, volatilization, or decomposition. Before the process its specific gravity is such that it does not sink in sulphuric acid; but after, it rapidly falls to the bottom in this fluid.

The phenomena of its combustion, are best witnessed in a retort filled with oxygene gas. When the bottom of the retort is gently heated by a spirit lamp, it throws off most vivid scintillations like those from the combustion of the bark of charcoal, and the mass burns with a brilliant light. A sublimate rises from it, which is boracic acid; and it becomes coated with a vitreous substance, which proves likewise to be boracic acid; and after this has been washed off, the residuum appears perfectly black, and requires a higher temperature for its inflammation than the olive coloured substance; and by its inflammation produces a fresh portion of boracic acid.

In oxymuriatic acid gas, the peculiar inflammable substance occasions some beautiful phenomena. When this gas is brought in contact with it at common temperatures, it instantly takes fire and burns with a brilliant white light, a white substance coats the interior of the vessel in which the experiment is made, and the peculiar substance is found covered by a white film, which by washing affords boracic acid, and leaves a black matter, which is not spontaneously inflammable in a fresh portion of the gas; but which inflames in it by a gentle heat, and produces boracic acid.

The peculiar inflammable substance, when heated nearly to redness in hydrogene, or nitrogene, did not seem to dissolve in these gasses, or to act upon them; it merely gained a darker shade of colour, and a little moisture rose from it, which condensed in the neck of the retort in which the experiment was made.

On the fluid menstrua containing oxygene, it produced effects which might be looked for from the phenomena of its agency on gasses.

When thrown into concentrated nitric acid, it rendered it bright red, so that nitrous gas was produced and absorbed, but it did not dissolve rapidly till the acid was heated; when there was a considerable effervescence, the peculiar substance disappeared, nitrous gas was evolved, and the fluid afforded boracic acid.

It did not act upon concentrated sulphuric acid till heat was applied; it then produced a slight effervescence; the acid became black at its points of contact with the solid; and a deep brown solution was formed, which, when neutralized by potash, gave a black precipitate.

When heated in a strong solution of muriatic acid, it gave it a faint tint of green; but there was no vividness of action, or considerable solution.

On acetic acid heated, it had no perceptible action.

It combined with the fixed alkalies, both by fusion and aqueous solution, and formed pale olive coloured compounds, which gave dark precipitates when decomposed by muriatic acid.

When it was kept long in contact with sulphur in fusion, it slowly dissolved, and the sulphur acquired an olive tint. It

was still less acted upon by phosphorus, and after an hour's exposure to it, had scarcely diminished in quantity, but the phosphorus had gained a tint of pale green.

It did not combine with mercury, when they were heated together.

These circumstances are sufficient to shew, that the combustible substance obtained from boracic acid by the agency of potassium, is different from any other known species of matter, and it seems, as far as the evidence extends, to be the same as that procured from it by electricity; and the two series of facts, seem fully to establish the decomposition, and recomposition of the acid.

From the large quantity of potassium required to decompose a small quantity of the acid, it is evident that the boracic acid must contain a considerable proportion of oxygene, I have endeavoured to determine the relative weights of the peculiar inflammable matter and oxygene, which compose a given weight of boracic acid; and to this end I made several analytical and synthetical experiments; I shall give the results of the two which I consider as most accurate.

Twenty grains of boracic acid and thirty grains of potassium, were made to act upon each other by heat in a tube of brass; the result did not effervesce when washed with diluted muriatic acid; and there were obtained after the process, by slight lixiviation in warm water, two grains and about $\frac{6}{15}$ of the olive coloured matter. Now thirty grains of potassium, would require about five grains of oxygene, to form thirty-five of potash; and according to this estimation, boracic acid must consist of about one of the peculiar inflammable substance, to nearly two of oxygene.

A grain of the inflammable substance in very fine powder, and diffused over a large surface, was set fire to in a retort, containing twelve cubical inches of oxygene; three cubical inches of gas were absorbed, and the black residuum collected after the boracic acid had been dissolved, was found to equal five eighths of a grain. This, by a second combustion, was almost entirely converted into boracic acid, with the absorption of two cubical inches and one eighth more of oxygene. The thermometer in this experiment was at $58°$ FAHRENHEIT, and the barometer at 30.2.

According to this result, boracic acid would consist of one of the inflammable matter, to about 1.8 of oxygene; and the dark residual substance, supposing it to be simply the inflammable matter combined with less oxygene than is sufficient to constitute boracic acid, would be an oxide, consisting of about 4.7 of inflammable matter, to 1.55 of oxygene.

These estimations, I do not however venture to give, as entirely correct. In the analytical experiments, there are probably sources of error, from the solution of a part of the inflammable matter, and it possibly may retain alkali, which cannot be separated by the acid. In the synthetical process, in which washing is employed, and so small a quantity of matter used, the results are still less to be depended upon; they must be considered only as imperfect approximations.

From the general tenour of the facts, it appears that the combustible matter obtained from boracic acid, bears the same relation to that substance, as sulphur and phosphorus do to the sulphuric and phosphoric acids. But is it an elementary

inflammable body, the pure basis of the acid ? or is it not like sulphur and phosphorus, compounded ?

Without entering into any discussion concerning ultimate elementary matter, there are many circumstances which favour the idea, that the dark olive substance, is not a simple body; its being non-conducting, its change of colour by being heated in hydrogene gas, and its power of combining with the alkalies; for these properties in general belong to primary compounds, that are known to contain oxygene.

I heated the olive coloured substance with potassium, there was a combination, but without any luminous appearance, and a gray metallic mass was formed; but from the effect of this upon water, I could not affirm that any oxygene had been added to the metal, the gas given off had a peculiar smell, and took up more oxygene by detonation than pure hydrogene, from which it seems probable, that it held some of the combustible matter in solution.

It occurred to me, that if the pure inflammable basis were capable of being deoxygenated by potassium, it would probably possess a stronger affinity for oxygene, than hydrogene, and therefore be again brought to its former state by water. I made another experiment on the operation of potassium, on the olive coloured substance, and exposed the mixture to a small quantity of ether, hoping that this might contain only water enough to oxygenate the potassium; but the same result occurred as in the last case; and a combination of potash and the olive coloured substance was produced, insoluble in ether.

I covered a small globule of potassium, with four or five times its weight of the olive coloured matter, in a platina tube

exhausted, after being filled with hydrogene; and heated the mixture to whiteness: no gas was evolved. When the tube was cooled, naphtha was poured into it, and the result examined under naphtha. Its colour was of a dense black. It had a lustre scarcely inferior to that of plumbago. It was a conductor of electricity. A portion of it thrown into water, occasioned a slight effervescence; and the solid matter separated, appeared dark olive, and the water became slightly alkaline. Another portion examined, after being exposed to air for a few minutes, had lost its conducting power, was brown on the surface, and no longer produced an effervescence in water.

Some of the olive inflammable matter, with a little potassium, was heated to whiteness, covered with iron filings, a dark metalline mass was formed, which conducted electricity, and which produced a very slight effervescence in water, and gave by solution in nitric acid, oxide of iron and boracic acid.

The substance which enters into alloy with potassium, and with iron, I am inclined to consider, as the true basis of the boracic acid.

In the olive coloured matter, this basis seems to exist in union with a little oxygene; and when the olive coloured substance is dried at common temperatures, it likewise contains water.

In the black non-conducting matter, produced in the combustion of the olive coloured substance, the basis is evidently combined with much more oxygene, and in its full state of oxygenation, it consitutes boracic acid.

From the colour of the oxides, their solubility in alkalies,

and from their general powers of combination, and from the conducting nature and lustre of the matter produced by the action of a small quantity of potassium upon the olive coloured substance, and from all analogy; there is strong reason to consider the boracic basis as metallic in its nature, and I venture to propose for it the name of *boracium.*

7. *Analytical Inquiries respecting Fluoric Acid.*

I have already laid before the Society, the account of my first experiments on the action of potassium, on fluoric acid gas.*

I stated, that the metal burns when heated in this elastic fluid, and that there is a great absorption of the gas.

Since the time that this communication was made, I have carried on various processes, with the view of ascertaining, accurately, the products of combustion, and I shall now describe their results.

When fluoric acid gas, that has been procured in contact with glass, is introduced into a plate glass retort, exhausted after being filled with hydrogene gas, white fumes are immediately perceived. The metal loses its splendour, and becomes covered with a grayish crust.

When the bottom of the retort is gently heated, the fumes become more copious; they continue for some time to be emitted, but at last cease altogether.

* Phil. Trans. Part II. 1808, p. 343. The combustion of potassium in fluoric acid, I have since seen mentioned in the number of the Moniteur, already so often quoted, as observed by M. M. Gay Lussac and Thenard; but no notice is taken of the results.

If the gas is examined at this time, its volume is found to be a little increased, by the addition of a small quantity of hydrogene.

No new fumes are produced by a second application of a low heat; but when the temperature is raised nearly to the point of sublimation of potassium, the metal rises through the crust, becomes first of a copper colour and then of a bluish black, and soon after inflames and burns with a most brilliant red light.

After this combustion, either the whole or a part of the fluoric acid, according as the quantity of potassium is great or small, is found to be destroyed or absorbed. A mass of a chocolate colour remains in the bottom of the retort; and a sublimate, in some parts chocolate, and in others yellow, is found round the sides, and at the top of the retort.

When the residual gas afforded by this operation, is washed with water, and exposed to the action of an electrical spark mixed with oxygene gas, it detonates and affords a diminution, such as might be expected from hydrogene gas.

The proportional quantity of this elastic fluid, differs a little in different operations. When the fluoric acid has not been artificially dried, it amounts to one sixth or one seventh of the volume of the acid gas used; but when the fluoric acid has been long exposed to calcined sulphate of soda, it seldom amounts to one tenth.

I have endeavoured to collect large quantities of the chocolate coloured substance for minute examination; but some difficulties occurred.

When I used from eighteen to twenty grains of potassium, in a retort containing from twenty to thirty cubical inches of

fluoric acid gas, the intensity of the heat was such, as to fuse the bottom of the retort, and destroy the results.

In a very thick plate glass retort, containing about nineteen cubical inches of gas, I once succeeded in making a decisive experiment on ten grains and a half of potassium, and I found that about fourteen cubical inches of fluoric acid disappeared, and about two and a quarter of hydrogene gas were evolved. The barometer stood at 30.3, and the thermometer at 61° FAHRENHEIT; the gas had not been artifically dried. In this experiment there was very little sublimate; but the whole of the bottom of the retort was covered with a brown crust, and near the point of contact with the bottom, the substance was darker coloured, and approaching in its tint to black.

When the product was examined by a magnifier, it evidently appeared consisting of different kinds of matter; a blackish substance, a white, apparently saline substance, and a substance having different shades of brown and fawn colour.

The mass did not conduct electricity, and none of its parts could be separated, so as to be examined as to this property.

When a portion of it was thrown into water, it effervesced violently, and the gas evolved had some resemblance in smell to phosphuretted hydrogene, and was inflammable.

When a part of the mass was heated in contact with air, it burnt slowly, lost its brown colour, and became a white saline mass.

When heated in oxygene gas, in a retort of plate glass, it absorbed a portion of oxygene, but burnt with difficulty, and required to be heated nearly to redness; and the light given

out was similar to that produced by the combustion of liver of sulphur.

The water which had acted upon a portion of it was examined; a number of chocolate coloured particles floated in it. When the solid matter was separated by the filter, the fluid was found to contain fluate of potash, and potash. The solid residuum was heated in a small glass retort in oxygene gas; it burnt before it had attained a red heat, and became white. In this process, oxygene was absorbed, and acid matter produced. The remainder possessed the properties of the substance formed from fluoric acid gas holding siliceous earth in solution, by the action of water.

In experiments made upon the combustion of quantities of potassium equal to.from six to eleven grains, the portion of matter separable from the water has amounted to a very small part of a grain only, and operating upon so minute a scale, I have not been able to gain fully decided evidence, that the inflammable part of it is the pure basis of the fluoric acid; but with respect to the decomposition of this body by potassium, and the existence of its basis at least combined with a smaller proportion of oxygene in the solid product generated, and the regeneration of the acid by the ignition of this product in oxygene gas, it is scarcely possible to entertain a doubt.

The decomposition of the fluoric acid by potassium, seems analogous to that of the acids of sulphur and phosphorus. In neither of these cases are the pure bases, or even the bases in their common form evolved; but new compounds result, and in one case sulphurets, and sulphites, and in the other phosphurets, and phosphites of potash, are generated.

As silex was always obtained during the combustion of the

chocolate coloured substance obtained by lixiviation, it oc-
curred to me that this matter might be a result of the ope-
ration, and that the chocolate substance might be a compound
of the siliceous and fluoric bases in a low state of oxygenation,
with potash; and this idea is favoured by some trials that I
made to separate silex from the mass, by boiling it in con-
centrated fluoric acid; the substance did not seem to be
much altered by this process, and still gave silex by com-
bustion.

I endeavoured to decompose fluoric acid gas in a perfectly
dry state, and which contained no siliceous earth; and for this
purpose I made a mixture of one hundred grains of dry bo-
racic acid, and two hundred grains of fluor spar, and placed
them in the bottom of an iron tube, having a stop-cock and
a tube of safety attached to it.

The tube was inserted horizontally in a forge, and twenty
grains of potassium, in a proper iron tray, introduced into
that part of it where the heat was only suffered to rise to dull
redness. The bottom of the tube was heated to whiteness,
and the acid acted upon by the heated potassium, as it was
generated. After the process was finished, the result in the
tray was examined.

It was in some parts black, and in others of a dark brown.
It did not effervesce with water: and when lixiviated, afforded
a dark brown combustible mass, which did not conduct elec-
tricity, and which when burnt in oxygene gas, afforded boracic,
and fluoric acid. It dissolved with violent effervescence in
nitric acid; but did not inflame spontaneously in oxymuriatic
acid gas.

. I have not as yet examined any of the other properties of

this substance; but I am inclined to consider it as a compound of the olive coloured oxide of boracium, and an oxide of the fluoric basis.

In examining the dry fluoric acid gas, procured in a process similar to that which has been just described, it gave very evident marks of the presence of boracic acid.

As the chocolate coloured substance is permanent in water, it occurred to me that it might possibly be producible from concentrated liquid fluoric acid at the negative surface in the VOLTAIC circuit.

I made the experiment with platina surfaces, from a battery of two hundred and fifty plates of six inches, on fluoric acid the densest that could be obtained by the distillation of fluor spar and concentrated sulphuric acid of commerce, in vessels of lead. Oxygene and hydrogene were evolved, and a dark brown matter separated at the deoxydating surface; but the result of an operation conducted for many hours, merely enabled me to ascertain that it was combustible, and produced acid matter in combustion; but I cannot venture to draw the conclusion that this acid was fluoric acid, as it was not impossible that some sulphureous, or sulphuric acid might likewise exist in the solution.

I heated the olive coloured inflammable substance, obtained from the boracic acid in common fluoric acid gas in a plate glass retort; the temperature was raised till the glass began to fuse; but no change, indicating a decomposition, took place.

I heated six grains of potassium with four grains of powdered fluor spar in a green glass tube filled with hydrogene; a minute quantity of hydrogene

gas was evolved, and a dark gray mass was produced, which acted upon water with much effervescence, but left no solid inflammable residuum.

8. *Analytical Experiments on Muriatic Acid.*

I have made a greater number of experiments upon this substance, than upon any of the other subjects of research that have been mentioned; it will be impossible to give any more than a general view of them within the limits of the Bakerian lecture.

Researches carried on some years ago, and which are detailed in the Journals of the Royal Institution, shewed that there were little hopes of decomposing muriatic acid, in its common form, by VOLTAIC electricity. When aqueous solution of muriatic acid is acted upon, the water alone is decomposed; and the VOLTAIC electrization of the gas affords no indications of its decomposition; and merely seems to shew, that this elastic fluid contains much more water than has been usually suspected.

I have already laid before the Society, an account of some experiments made on the action of potassium on muriatic acid. I have since carried on the same processes on a larger scale, but with precisely similar results.

When potassium is introduced into muriatic acid gas, procured from muriate of ammonia and concentrated sulphuric acid, and freed from as much moisture as muriate of lime is capable of attracting from it, it immediately becomes covered with a white crust, it heats spontaneously, and by the assistance of a lamp, acquires in some parts the temperature of ignition, but does not inflame. When the potassium and the gas are in

proper proportions, they both entirely disappear; a white salt is formed, and a quantity of pure hydrogene gas evolved, which equals about one third of the original volume of the gas.

By eight grains of potassium· employed in this way, I effected the absorption of nearly twenty two cubical inches of muriatic acid gas; and the quantity of hydrogene gas produced was equal to more than eight cubical inches.

The correspondence between the quantity of hydrogene generated in cases of this kind, and by the action of potassium upon water, combined with the effects of ignited charcoal upon muriatic acid gas, by which a quantity of inflammable gas is produced equal to more than one third of its volume; seemed to shew, that the phenomena merely depended upon moisture combined with the muriatic acid gas.*

To determine this point with more certainty however, and to ascertain whether or no the appearance of the hydrogene was wholly unconnected with the decomposition of the acid, I made two comparative experiments on the quantity of muriate of silver, furnished by two equal quantities of muriatic acid, one of which had been converted into muriate of potash by the action of potassium, and the other of which had been absorbed by water; every care was taken to avoid sources of error; and it was found that there was no notable difference in the weight of the results.

* When the VOLTAIC spark is taken continuously, by means of points of charcoal in muriatic acid gas over mercury, muriate of mercury is rapidly formed, a volume of inflammable gas, equal to one third of the original volume of the muriatic acid gas appears. The acid gas enters into combination with the oxide of mercury, so that water enough is present in the experiment to form oxide sufficient to absorb the whole of the acid.

There was no proof then, that the muriatic acid had been decompounded in these experiments; and there was every reason to consider it as containing in its common aeriform state, at least one third of its weight of water; and this conclusion we shall find warranted by facts, which are immediately to follow.

I now made a number of experiments, with the hopes of obtaining the muriatic acid free from water.

I first heated to whiteness, in a well luted porcelain retort, a mixture of dry sulphate of iron, and muriate of lime which had been previously ignited; but a few cubic inches of gas only, were obtained, though the mixture was in the quantity of several ounces; and this gas contained sulphureous acid. I heated dry muriate of lime, mixed both with phosphoric glass and dry boracic acid, in tubes of porcelain, and of iron, and employed the blast of an excellent forge; but by neither of these methods was any gas obtained, though when a little moisture was added to the mixtures, muriatic acid was develloped in such quantities, as almost to produce explosions.

The fuming muriate of tin, *the liquor of Libavius*, is known to contain dry muriatic acid. I attempted to separate the acid from this substance, by distilling it with sulphur and with phosphorus; but without success. I obtained only triple compounds, in physical characters, something like the solutions of phosphorus, and sulphur in oil, which were non-conductors of electricity, which did not redden dry litmus paper, and which evolved muriatic acid gas with great violence, heat, and ebullition on the contact of water.

I distilled mixtures of corrosive sublimate and sulphur, and of calomel and sulphur; when these were used in their

common states, muriatic acid gas was evolved; but when they were dried by a gentle heat, the quantity was exceedingly diminished, and the little gas that was generated gave hydrogene by the action of potassium. During the distillation of corrosive sublimate and sulphur, a very small quantity of a limpid fluid passed over. When examined by transmitted light, it appeared yellowish green. It emitted fumes of muriatic acid, did not redden dry litmus paper, and deposited sulphur by the action of water. I am inclined to consider it as a modification of the substance discovered by Dr. Thomson, in his experiments on the action of oxymuriatic acid on sulphur.

M. M. Gay Lussac and Thenard * have mentioned, that they endeavoured to procure dry muriatic acid by distilling a mixture of calomel and phosphorus, and that they obtained a fluid which they consider as a compound of muriatic acid, phosphorus, and oxygene. In distilling corrosive sublimate with phosphorus, I had a similar result, and I obtained the substance in much larger quantities, than by the distillation of phosphorus with calomel.

As oxymuriatic acid is slightly soluble in water, there was reason to suppose, reciprocally that water must be slightly soluble in this gas; I endeavoured therefore to procure dry muriatic acid, by absorbing the oxygene from oxymuriatic acid gas by substances, which when oxygenated, produce compounds possessing a strong affinity for water. Phosphorus, it is well known, burns in oxymuriatic acid gas; though the results of this combustion, I believe, have never been minutely examined. With the hopes of procuring muriatic acid gas, free from moisture, I made the experiment.

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* The Moniteur before quoted.

I introduced phosphorus into a receiver having a stop-cock, which had been exhausted, and admitted oxymuriatic acid gas. As soon as the retort was full, the phosphorus entered into combustion, throwing forth pale white flames. A white sublimate collected in the top of the retort, and a fluid as limpid as water, trickled down the sides of the neck. The gas seemed to be entirely absorbed, for when the stop-cock was opened, a fresh quantity of oxymuriatic acid, nearly as much as would have filled the retort, entered.

The same phenomenon of inflammation again took place, with similar results. Oxymuriatic acid gas was admitted till the whole of the phosphorus was consumed.

Minute experiments proved, that no gaseous muriatic acid had been evolved in this operation, and the muriatic acid was consequently to be looked for either in the white sublimate, or in the fluid which had formed in the neck of the retort.

The sublimate was in large portions, the fluid only in the quantity of a few drops. I collected by different processes, sufficient of both for examination.

The sublimate emitted fumes of muriatic acid when exposed to air. When brought in contact with water, it evolved muriatic acid gas, and left phosphoric acid, and muriatic acid, dissolved in the water. It was a non-conductor of electricity, and did not burn when heated; but sublimed when its temperature was about that of boiling water, leaving not the slightest residuum. I am inclined to regard it as a combination of phosphoric, and muriatic acid in their dry states.

The fluid was of a pale greenish yellow tint, and very limpid; when exposed to air, it rapidly disappeared, emitting dense

white fumes which had a strong smell differing a little from that of muriatic acid.

It reddened litmus paper in its common state, but had no effect upon litmus paper which had been well dried, and which was immediately dipped into it. It was a non-conductor of electricity. It heated when mixed with water, and evolved muriatic acid gas. I consider it as a compound of phosphorous acid, and muriatic acid, both free from water.*

Having failed in obtaining uncombined muriatic acid in this way, I performed a similar process with sulphur, but I was unable to cause it to inflame in oxymuriatic acid gas. When it was heated in it, it produced an orange coloured liquid, and yellow fumes passed into the neck of the retort, which condensed into a greenish yellow fluid. By repeatedly passing oxymuriatic acid through this fluid, and distilling it several times in the gas, I rendered it of a bright olive colour, and in this case it seemed to be a compound of dry sulphuric, and muriatic acid, holding in solution a very little sulphur. When it was heated in contact with sulphur, it rapidly dissolved it, and then became of a bright red colour, and when saturated with sulphur, of a pale golden colour.† No permanent aeriform fluid was evolved in any of these operations, and no muriatic gas appeared, unless moisture was introduced.

As there seemed little chance of procuring uncombined

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* I attempted to obtain dry muriatic acid likewise from the phosphuretted muriatic acid of M. M. GAY LUSSAC and THENARD, by distilling it in retorts containing oxygene gas, and oxymuriatic acid gas. In the first case, the retort was shattered by the combustion of the phosphorus, with a violent explosion. In the second, compounds, similar to those described above, were formed.

† All these substances seem to be of the same nature as the singular compound, the sulphuretted muriatic acid, discovered by Dr. THOMSON, noticed in page 93.

muriatic acid, it was desirable to ascertain what would be the effects of potassium upon it in these singular compounds.

When potassium was introduced into the fluid, generated by the action of phosphorus on corrosive sublimate, at first it slightly effervesced, from the action of the liquid on the moist crust of potash surrounding it ; but the metal soon appeared perfectly splendid, and swimming on the surface. I attempted to fuse it by heating the fluid, but it entered into ebullition at a temperature below that of the fusion of the potassium ; indeed the mere heat of the hand was sufficient for the effect. On examining the potassium, I found that it was combined at the surface with phosphorus, and gave phosphuretted hydrogene by its operation upon water.

I endeavoured, by repeatedly distilling the fluid from potassium in a close vessel, to free it from phosphorus, and in this way I succeeded in depriving it of a considerable quantity of this substance

I introduced ten or twelve drops of the liquid, which had been thus treated, into a small plate glass retort, containing six grains of potassium ; the retort was exhausted after having been twice filled with hydrogene, the liquid was made to boil, and the retort kept warm till the whole had disappeared as elastic vapour. The potassium was then heated by the point of a spirit lamp ; it had scarcely melted, when it burst into a most brilliant flame, as splendid as that of phosphorus in oxygene gas, and the retort was destroyed by the rapidity of combustion.

In other trials made upon smaller quantities after various failures, I was at last able to obtain the results ; there was no proof of the evolution of any permanent elastic fluid during

the operation. A solid mass remained of a greenish colour at the surface, but dark gray in the interior. It was extremely inflammable, and often burnt spontaneously when exposed to air; when thrown upon water, it produced a violent explosion, with a smell like that of phosphuretted hydrogene. In the residuum of its combustion there was found muriate of potash, and phosphate of potash.

I endeavoured to perform this experiment in an iron tube, hoping that if the muriatic acid was decomposed in the process, its inflammable element, potassium and phosphorus, might be separated from each other by a high degree of heat; but in the first part of the operation the action was so intense, as to produce a destruction of the apparatus, and the stop-cock was separated from the tube with a loud detonation.

I heated potassium in the vapour of the compound of muriatic and phosphoric acid; but in this case, the inflammation was still more intense, and in all the experiments that I have hitherto tried, the glass vessels have been either fused or broken; the solid residuum has however appeared to be of the same kind as that I have just described.

The results of the operation of the sulphuretted compounds containing muriatic acid free from water upon potassium, are still more extraordinary than those of the phosphuretted compounds.

When a piece of potassium is introduced into the substance that distils over during the action of heated sulphur upon oxymuriatic acid, it at first produces a slight effervescence, and if the volume of the potassium considerably exceeds that of the liquid, it soon explodes with a violent report, and a most intense light.

I have endeavoured to collect the results of this operation, by causing the explosion to take place in large exhausted plate glass retorts; but, except in a case in which I used only about a quarter of a grain, I never succeeded. Generally the retort, though connected with the air pump at the time, was broken into atoms; and the explosion produced by a grain of potassium, and an equal quantity of the fluid, has appeared to me considerably louder than that of a musket.

In the case in which I succeeded in exploding a quarter of a grain, it was not possible for me to ascertain if any gaseous matter was evolved; but a solid compound was formed of a very deep gray tint, which burnt, throwing off bright scintillations, when gently heated, which inflamed when touched with water, and gave most brilliant sparks, like those thrown off by iron in oxygene gas.

Its properties certainly differed from those of any compound of sulphur and potassium that I have seen : whether it contains the muriatic basis must however be still a matter of inquiry.

There is, however, much reason for supposing, that in the singular phenomena of inflammation and detonation that have been described, the muriatic acid cannot be entirely passive: and it does not seem unfair to infer, that the transfer of its oxygene and the production of a novel substance, are connected with such effects, and that the highly inflammable nature of the new compounds, partly depends upon this circumstance. I am still pursuing the inquiry, and I shall not fail immediately to communicate to the Society, such results as may appear to me worthy of their attention.

9. *Some general Observations, with Experiments.*

An experiment has been lately published, which appeared so immediately connected with the discussion entered into in the second section of this Paper, that I repeated it with much earnestness.

In Mr. NICHOLSON's Journal for December, Dr. WOODHOUSE has given an account of a process, in which the action of water caused the inflammation of a mixture of four parts of charcoal and one of pearlash that had been strongly ignited together, and the emission of ammonia from them. I thought it possible, that in this case a substance might be formed similar to the residuum described in page 50; but by cooling the mixture out of the contact of nitrogene, I found that no ammonia was formed; and this substance evidently owed its existence to the absorption of atmospherical air by the charcoal.*

The experiments that I have detailed on the acids, offer some new views with respect to the nature of acidity. That a compound of muriatic acid with oxide of tin or phosphorus should

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* Potash or pearlash is easily decomposed by the combined attractions of charcoal and iron; but it is not decomposable by charcoal, or, when perfectly dry, by iron alone. Two combustible bodies seem to be required by their combined affinities for the effect; thus in the experiment with the gun barrel, iron and hydrogene are concerned. I consider HOMBERG's pyrophorus as a triple compound of potassium, sulphur, and charcoal; and in this ancient process, the potash is probably decomposed by two affinities. The substance is perfectly imitated by heating together ten parts of charcoal, two of potassium, and one of sulphur.

When I first shewed the production of potassium to Dr. WOLLASTON in October 1807, he stated, that this new fact induced him to conceive that the action of potash upon platina, was owing to the formation of potassium, and proposed it, as a matter of research, whether the alkali might not be decomposed by the joint action of platina and charcoal.

not redden vegetable blues, might be ascribed to a species of neutralization, by the oxide or inflammable body; but the same reasoning will not apply to the dry compounds which contain acid matter only, and which are precisely similar as to this quality. Let a piece of dry and warm litmus paper be moistened with the compound of muriatic and phosphorous acid, it perfectly retains its colour. Let it then be placed upon a piece of moistened litmus paper, it instantly becomes of a bright red, heats and devellopes muriatic acid gas.

All the fluid acids that contain water are excellent conductors of electricity, in the class called that of imperfect conductors; but the compounds to which I have just alluded, are non-conductors in the same degree as oils, with which they are perfectly miscible. When I first examined muriatic acid, in its combinations free from moisture, I had great hopes of decomposing them by electricity; but there was no action without contact of the wires, and the spark seemed to separate no one of their constituents, but only to render them gaseous. The circumstance likewise applies to the boracic acid, which is a good conductor as long as it contains water; but which, when freed from water and made fluid by heat, is then a non-conductor.

The alkalies and the earthy compounds, and the oxides, as dry as we can obtain them, though non-conductors when solid, are, on the contrary, all conductors when rendered fluid by heat.

When muriatic acid, existing in combination with phosphorous or phosphoric acid, is rendered gaseous by the action of water, the quantity of this fluid that disappears, at least equals from one third to two fifths of the weight of the acid gas

produced; a circumstance that agrees with the indications given by the action of potassium.*

I attempted to procure a compound of dry muriatic and carbonic acids, hoping that it might be gaseous, and that the two acids might be decomposable at the same time by potassium. The process that I employed was by passing corrosive sublimate in vapour through charcoal ignited to whiteness; but I obtained a very small quantity of gas, which seemed to be a mixture of common muriatic acid gas and carbonic acid gas; a very minute portion of running mercury only was obtained, by a long continuation of the process; and the slight decomposition that did take place, I am inclined to attribute to the production of water, by the action of the hydrogene of the charcoal upon the oxygene of the oxide of mercury.†

In mixing muriatic acid gas with carbonic acid, or oxygene, or hydrogene, the gases being in their common states, as to moisture, there was always a cloudiness produced; doubtless owing to the attraction of their water to form liquid muriatic acid.

On fluoric acid gas no such effect was occasioned. This fact, at first view, might be supposed to shew, that the hydrogene evolved by the action of potassium upon fluoric acid gas, is

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* Page 98.

† These facts and the other facts of the same kind, explain the difficulty of the decomposition of the metallic muriates in common processes of metallurgy. They likewise explain other phenomena in the agencies of muriatic salts. In all cases when a muriatic salt is decomposed by an acid, and muriatic acid gas set free, there appears to be a double affinity, that of the acid for the basis, and of the muriatic acid for water; pure muriatic acid does not seem capable of being displaced by any other acid.

owing to water in actual combination with it, like that in muriatic acid gas, and which may be essential to its elastic state; but it is more probable, from the smallness of the quantity, and from the difference of the quantity in different cases, that the moisture is merely in that state of diffusion or solution in which it exists in gases in general, though from the disposition of water to be deposited in this acid gas in the form of an acid solution, it must be either less in quantity, or in a less free state, so as to require for its exhibition much more delicate hygrometrical tests.

The facts advanced in this Lecture, afford no new arguments in favour of an idea to which I referred in my last communication to the Society, that of hydrogene being a common principle in all inflammable bodies; and except in instances which are still under investigation, and concerning which no precise conclusions can as yet be drawn, the generalization of LAVOISIER happily applies to the explanation of all the new phenomena.

In proportion as progress is made towards the knowledge of pure combustible bases, so in proportion is the number of metallic substances increased; and it is probable that sulphur and phosphorus, could they be perfectly deprived of oxygene, would belong to this class of bodies. Possibly their pure elementary matter may be procured by distillation, at a high heat, from metallic alloys, in which they have been acted upon by sodium or potassium. I hope soon to be able to try this experiment.

As our inquiries at present stand, the great general division of natural bodies is into matter which is, or may be supposed to be, metallic, and oxygene; but till the problem concerning

the nature of nitrogene is fully solved, all systematic arrangements made upon this idea must be regarded as premature.

EXPLANATION OF THE FIGURES.

Fig. 1. The retort of plate glass for heating potassium in gases.

Fig. 2. The tray of platina for receiving the potassium.

Fig. 3. The platina tube for receiving the tray in experiments of distillation.

Fig. 4. The apparatus for taking the VOLTAIC spark in sulphur and phosphorus.
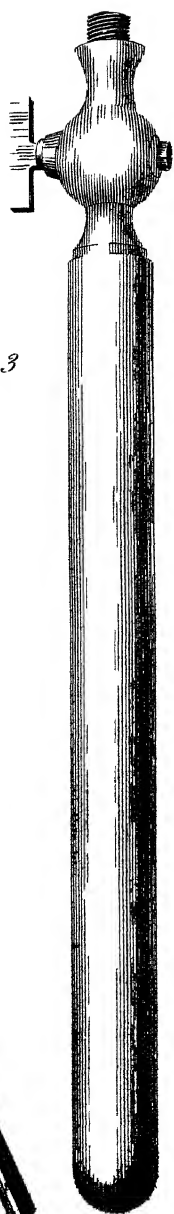
Fig 3
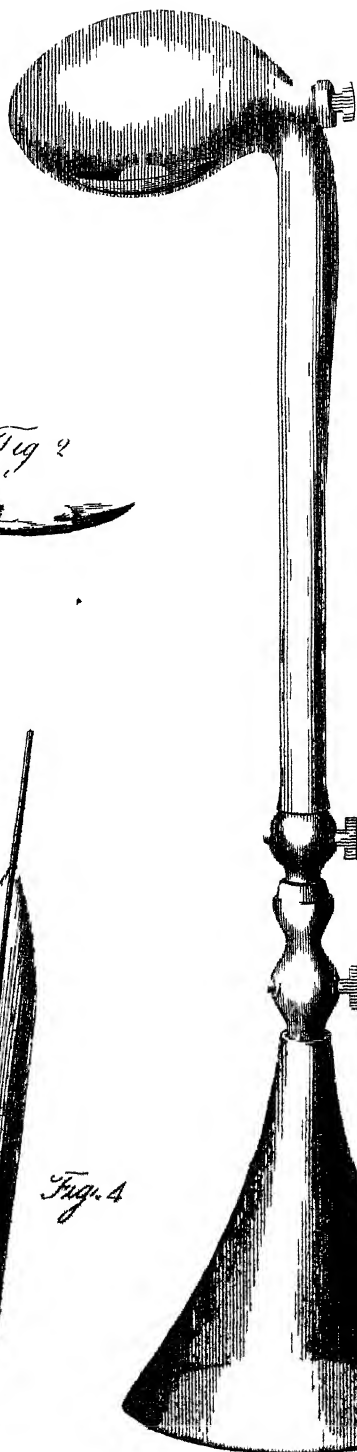
Fig 1

Fig 2
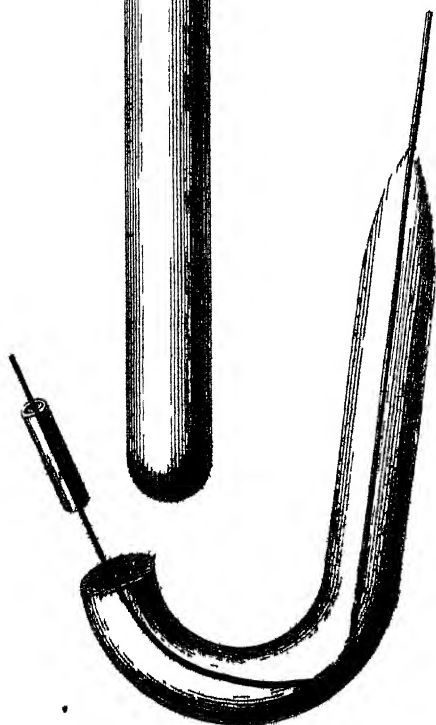
Fig. 4

IV. *An Account of a Method of dividing Astronomical and other Instruments, by ocular Inspection; in which the usual Tools for graduating are not employed; the whole Operation being so contrived, that no Error can occur but what is chargeable to Vision, when assisted by the best optical Means of viewing and measuring minute Quantities. By Mr.* Edward Troughton. *Communicated by the Astronomer Royal.*

Read February 2, 1809.

IT would ill become me, in addressing myself to the Members of this Society upon a subject which they are so well enabled to appreciate, to arrogate to myself more than may be assigned as my due, for whatever of success may have been the result of my long continued endeavours, exerted in prosecuting towards perfection *the dividing of Instruments immediately subservient to the purposes of Astronomy.* A man very naturally will set a value upon a thing on which so much of his life has been expended; and I shall readily, therefore, be pardoned for saying, that considering some attainments which I have made on this subject as too valuable to be lost, and being encouraged also by the degree of attention which the Royal Society has ever paid to practical subjects, I feel myself ambitious of presenting them to the public through what I deem the most respectable channel in the world.

It was as early as the year 1775, being then apprentice to my brother, the late Mr. JOHN TROUGHTON, that the art of

dividing had become interesting to me; the study of astronomy was also new and fascinating; and I then formed the resolution to aim at the nicer parts of my profession.

At the time alluded to, my brother, in the art of dividing, was justly considered the rival of RAMSDEN; but he was then almost unknown beyond the narrow circle of the mathematical and optical instrument makers; for whom he was chiefly occupied in the division, by hand, of small astronomical quadrants, and HADLEY's sextants of large radius. Notwithstanding my own employment at that time was of a much inferior nature, yet I closely inspected his work, and tried at leisure hours on waste materials to imitate it. With as steady a hand, and as good an eye, as young men generally have, I was much disappointed at finding, that after having made two points, neat and small to my liking, I could not bisect the distance between them, without enlarging, displacing, or deforming them with the points of the compasses. This circumstance gave me an early dislike to the tools then in use; and occasioned me the more uneasiness, as I foresaw that it was an evil which no practice, care, nor habit could entirely cure: Beam-compasses, spring-dividers, and a scale of equal parts, in short, appeared to me little better than so many sources of mischief.

I had already acquired a good share of dexterity, as a general workman: Of the different branches of our art, that of *turning* alone seemed to me to border on perfection. This juvenile conceit, fallacious as I afterwards found it, furnished the first train of thoughts, which led to the method about to be described; for it occurred to me, that if I could by any means apply the principle of turning to the art of dividing

instruments, the tools liable to objection might be dispensed with. The means of doing this was first suggested, by seeing the action of the perambulator, or measuring wheel; the surface of the earth presenting itself as the edge of the instrument to be divided, and the wheel of the perambulator as a narrow roller acting on that edge; and hence arose an idea that some easy contrivance might be devised, for marking off the revolutions and parts of the roller upon the instrument. Since the year above-mentioned, several persons have proposed to me, as new, dividing by the roller, and I have been told, that it also occurred long ago to HOOK, SISSON, and others; but, as HATTON on Watch-making, says, " I do not " consider the man an inventor, who merely thinks of a thing; " to be an inventor, in my opinion, he must act successfully " upon the thought, so as to make it useful." I had no occasion, however, to have made an apology for acting upon a thought, which, unknown to me, had been previously conceived by others; for it will be seen in the sequel, how little the roller has to do in the result, and with what extreme caution it is found necessary to employ it.

When a roller is properly proportioned to the radius of the circle to be divided, and with its edge made a small matter conical, so that one side may be too great, and the other side too little, it may be adjusted so exactly, that it may be carried several times around the circle, without the error of a single second; and it acts with so much steadiness, that it may not unaptly be considered as a wheel and pinion of indefinitely high numbers. Yet, such is the imperfection of the edges of the circle and roller, that, when worked with the greatest care, the intermediate parts, on a radius of two feet, will sometimes be

unequal to the value of half a minute or more. After having found the terminating point of a quadrant or circle so permanent, although I was not prepared to expect perfect equality throughout, yet I was much mortified to find the errors so great, at least ten times as much as I expected; which fact indicated, beyond a doubt, that if the roller is to be trusted at all, it must only be trusted through a very short arc. Had there been any thing slippery in the action, which would have been indicated by measuring the same part at different times differently, there would have been an end of it at once; but, that not being the case in any sensible degree, the roller becomes an useful auxiliary to fill up short intervals, whose limits have been corrected by more certain means.*

* There are two things in the foregoing account of the action of the roller which have a tendency to excite surprise. The first is, that the roller should, in different parts of its journey round the circle, measure the latter so differently. One would not wonder, however, if in taking the measure across a ploughed field, it should be found different to a parallel measure taken upon a gravel-walk; and, in my opinion, the cases are not very dissimilar. Porosity of the metal, in one part of the circle more than in the other, must evidently have the same effect; brass unhammered is always porous; and the part, which has felt the effect of two blows, cannot be so dense as other parts which have felt the effect of three; and, should the edge of the circle be indented by *jarring turning*, it would produce a visible similitude to ploughed ground: Every workman must be sufficiently upon his guard against such a palpable source of error; yet, perhaps with our greatest care we may not be able to avoid it altogether. The second is, that notwithstanding the inequality above-mentioned, the roller having reached the point upon the circle from whence it set out, should perform a second, third, &c. course of revolutions, without any sensible deviation from its former track; this is not perhaps so easily accounted for. It must be mentioned, that the exterior border of the circle should be *turned rounding,* presenting to the roller a convex edge, whose radius of curvature is not greater than one tenth of an inch. Now, were the materials perfectly inelastic and impenetrable, the roller could only touch the circle in a point, and in passing round the circle, it could only occupy a *line* of contact. This in practice is not the case; the circle always marks the roller with a

BIRD, who enjoyed the undisputed reputation of being the most accurate divider of the age in which he lived, was the first who contrived the means how to render the usual divisions of the quadrant bisectional; which property, except his being unusually careful in avoiding the effects of unequal expansion from change of temperature, chiefly distinguished his method from others who divided by hand. This desirable object he accomplished by the use which he made of a finely divided scale of equal parts. The thing aimed at was, to obtain a point upon the arc at the highest *bisectional number of divisions* from 0, which in his eight feet quadrants was 1024, $= 85°\ 20'$. The extent of the beam-compasses, with which he traced the arc upon the limb of the instrument to be divided, being set off upon that arc, gave the points 0° and 60°;

broad list, and thereby shews that there is a yielding between them to a considerable amount. The breadth of this list is not less than one fiftieth of an inch; and it follows, that at least 12° of the circle's edge must be in contact at the same time; that the two surfaces yield to each other in depth, by a quantity equal to the *ver. sin.* of half that arc, or $\frac{1}{1800}$ of an inch; and that the circle has always hold of the roller by nearly 1° of the edge of the latter. Whoever has examined the surfaces of metals which have rolled against each other, must have observed that peculiar kind of indentation that always accompanies their action; and there can be no doubt that the particles of a roller, and those of the surface on which it acts, which mutually indent each other, will, upon a second course begun from the same point, indent each other deeper: This is not, however, exactly the case in question; for, whatever of fitting might have taken place between the surfaces of our roller and circle in the first revolution of the former, one should imagine would be obliterated by the fifteen turns which it must repeat over fresh ground. Experience shows, however, as every one will find who tries the experiment with good work, that on coming round to the point of commencement, the roller has the disposition to regain its former track; for, were this not the case, although the commensurate diameters were adjusted so exactly as to be without sensible error in one course, yet a less error than that which is so would become visible, when repeated through many courses.

which, being bisected, gave 30° more to complete the total arc. A second order of bisections gave points at 15° distance from each other; but that which denoted 75° was most useful. Now, from the known length of the radius, as measured upon the scale, the length of the chord of 10° 20′ was computed, taken off from the scale, and protracted from 75° forwards; and the chord of 4° 40′, being ascertained in the same manner, was set off from 90° backwards, meeting the chord of 10° 20′ in the continually bisectional arc of 85° 20′. This point being found, the work was carried on by bisections, and the chords, as they became small enough, were set off beyond this point to supply the remainder of the quadrantal arc. My brother, whom I mentioned before, from mere want of a scale of equal parts upon which he could rely, contrived the means of dividing bisectionally without one. His method I will briefly state as follows, in the manner which it would apply to dividing a mural quadrant. The arcs of 60° and 30° give the total arc as before; and let the last arc of 30° be bisected, also the last arc of 15°, and again the last arc of 7° 30′: The two marks next 90° will now be 82° 30′ and 86° 15′, consequently the point sought lies between them. Bisections will serve us no longer; but if we divide this space equally into three parts, the most forward of the two intermediate marks will give us 85°, and if we divide the portion of the arc between this mark and 86° 15′ also into three, the most backward of the two marks will denote 85° 30′. Lastly, if we divide any one of these last spaces into five, and set off one of these fifth parts backwards from 85° 30′, we shall have the desired point at 1024 divisions upon the arc from 0°. All the rest of the divisions which have been made in this operation, which I have

called marks because they should be made as faint as possible, must be erased ; for my brother would not suffer a mark to remain upon the arc to interfere with his future bisections.

Mr. SMEATON, in a paper to be more particularly noticed presently, justly remarks the want of a unity of principle in Mr. BIRD's method ; for he proceeds partly on the ground of the protracted radius, and partly upon that of the computed chord ; which, as SMEATON observes, may or may not agree. BIRD, without doubt, used the radius and its parts in order to secure an exact quadrant; but SMEATON, treating exactness in the total arc as of little value to astronomy, would, in order to secure the more essential property of equality of division, reject the radius altogether, and proceed entirely upon the simple principle of the computed chord. The means pursued by my brother, to reach the point which terminates the great bisectional arc, is the only part in which it differs from BIRD's method ; and, I think it is without prejudice that I give it the preference. It is obvious that it is as well calculated to procure equality of division, as the means suggested by SMEATON ; at the same time that it is equal to BIRD's in securing the precise measure of the total arc. It proceeds entirely upon the principle of the protracted chord of 6o° and its subdivision; and the uncertainty, which is introduced into the work by the sparing use which is made of subdivision by 3 and 5, is, in my opinion, likely to be much exceeded by the errors of a divided scale,* and those of the hand and eye, in taking off the computed chords, and applying them to the arc of the instrument to be divided.

* That BIRD's scale was not without considerable errors, will be shewn towards the end of this paper.

RAMSDEN's well known method of dividing by the engine unites so much accuracy and facility, that a better can hardly be wished for; and I may venture to say that it will never be superseded, in the division of instruments of *moderate radii.* It was well suited to the time in which it appeared; a time when the improvements made in nautical astronomy, and the growing commerce of our country, called for a number of reflecting instruments, which never could have been supplied, had it been necessary to have divided them by hand; however, as it only applies to small instruments, it hardly comes within the subject of this paper.

The method of HINDLEY, as described by SMEATON,* I will venture to predict will never be put in practice for dividing astronomical instruments, however applicable it might formerly have been for obtaining numbers for cutting clockwork, for which purpose it was originally intended. It consists of a train of violent operations with blunt tools, any one of which is sufficient to stretch the materials beyond, or press them within their natural state of rest; and, although the whole is done by contact, the nature of this contact is such as, I think, ought rather to have been contrasted with, than represented as being similar to, the nature of the contact used in SMEATON's Pyrometer, which latter is performed by the most delicate touch; and is represented, I believe justly, to be sensible to the $\frac{1}{60000}$ part of an inch. SMEATON has, however, acquitted himself well, in describing and improving the method of his friend; and the world is particularly obliged to him for the historical part of his paper, as it contains valuable information which perhaps no one else could have written.

* Phil. Trans. for 1788.

The only method of dividing large instruments now prac-
tised in London, that I know of, besides my own, has not yet,
I believe, been made public. It consists in dividing by hand
with beam compasses and spring dividers, in the usual way ;
with the addition of examining the work by microscopes, and
correcting it, as it proceeds, by pressing forwards or back-
wards by hand, with a fine conical point, those dots which
appear erroneous; and thus adjusting them to their proper
places. The method admits of considerable accuracy, pro-
vided the operator has a steady hand and good eye ; but his
work will ever be irregular and inelegant. He must have a
circular line passing through the middle of his dots, to enable
him to make and keep them at an equal distance from the
centre. The bisectional arcs, also, which cut them across,
deform them much ; and, what is worse, the dots which re-
quire correction (about two thirds perhaps of the whole) will
become larger than the rest, and unequally so in proportion
to the number of attempts which have been found necessary
to adjust them. In the course of which operation, some of
them grow insufferably too large, and it becomes necessary
to reduce them to an equality with their neighbours. This is
done with the burnisher, and causes a hollow in the surface,
which has a very disagreeable appearance. Moreover, dots
which have been burnished up are always ill-defined, and of
a bad figure. Sir GEORGE SHUCKBURGH EVELYN, in his paper
on the Equatorial,* denominates these " doubtful or bad
points ;" and, (considering the few places which he examines)
they bear no inconsiderable proportion to the whole. In my
opinion, it would be a great improvement of this method, to

* Phil. Trans. for 1793.

divide the whole by hand at once, and afterwards to correct the whole; for a dot forced to its place, as above, will seldom allow the compass-point to rest in the centre of its apparent area; therefore other dots made from those will scarcely ever be found in their true places. This improvement also prevents the corrected dots from being injured, or moved, by the future application of the compasses, no such application being necessary.

I will now dismiss this method of dividing, with observing, that it is tedious in the extreme; and did I not know the contrary beyond a doubt, I should have supposed it to have surpassed the utmost limit of human patience.* When I made my first essay at subdividing with the roller, I used this method, according to the improvement suggested above, of correcting a few primitive points; but even this was too slow for one who had too much to do. Perhaps, however, had my instruments been divided for me by an assistant, I might not have grudged to have paid him for the labour of going through the whole work by the method of adjustment; nor have felt the necessity of contriving a better way.

I might now extend the account of my method of dividing to a great length; by relating the alterations which the

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* At the time alluded to, the double microscopic micrometer was unknown to me, and I did not learn its use, for these purposes, till the year 1790, from General ROY's description of the large theodolite. Previous to that time, I had used a frame which carried a single wire very near the surface to be divided; this wire was moveable by a fine micrometer screw, and was viewed by a single lens inserted in the lower end of a tube, which, for the purpose of taking off the parallax, was 4 inches long. The greatest objection to this mode of constructing the apparatus is, that the wire being necessarily exposed, is apt to gather up the dust; yet it is preferable to the one now in use, in cases where any doubt is entertained of the accuracy of the plane which is to receive the divisions.

apparatus has undergone during a long course of years,* and the various manner of its application, before I brought it to its present state of improvement; but I think I may save myself that trouble, for truly I do not see its use: I will, therefore, proceed immediately to a disclosure of the method, as practised on a late occasion, in the dividing of a four feet meridian circle, now the property of Stephen Groombridge, Esq. of Blackheath.

The surface of the circle which is to receive the divisions, as well as its inner and outer edges, but especially the latter, should be turned in the most exact and careful manner; the reason for which will be better understood, when we come to describe the mode of applying the roller: and, as no projection can be admitted beyond the limb, if the telescope, as is generally the case, be longer than the diameter, those parts which extend further must be so applied, that they may be removed during the operation of dividing. Fig. 1 and 2 represent the principal parts of the apparatus; Fig. 1 shewing the plan, and Fig. 2 the elevation; in both of which the same letters of reference are affixed to corresponding parts, and both are drawn to a scale of half dimensions. A A is a part of the circle, the surface of which is seen in the plan, and the edge is seen in the elevation. B B B is the main plate of the apparatus, resting with its four feet *a a a a* upon the surface of the arc; these feet, being screws, may be adjusted so as to take equal shares of the weight, and then are fastened by nuts below the plate,

* The full conception of the method had occupied my mind in the year 1778; but as my brother could not be readily persuaded to relinquish a branch of the business to me in which he himself excelled, it was not until September 1785 that I produced my first specimen, by dividing an astronomical quadrant of two feet radius.

as shewn in Fig. 2. C C and D D are two similar plates, each attached to the main plate, one above and the other below, by four pillars; and in them are centred the ends of the axis of the roller E. F and G are two friction wheels, the latter firmly fastened to B, but the former is fixed in an adjustable frame, by means of which adjustment these wheels and the roller E, may be made to press; the former on the interior, and the latter on the exterior edge of the circle, with an equal and convenient force.* At the extremities of the axis of the roller, and attached to the middle of the plates C and D, are two bridges, having a screw in each; by means of which an adjustment is procured for raising or lowering the roller respecting the edge of the circle, whereby the former, having its diameter at the upper edge about .001 of an inch greater than at the lower edge (being, as before described, a little conical), it may easily be brought to the position where it will measure the proper portion of the circle.

Much experience and thought upon the subject have taught me, that the roller should be equal to one sixteenth part of the circle to be divided, or that it should revolve once in 22° 30'; and that the roller itself should be divided into sixteen parts; no matter whether with absolute truth, for accuracy is not at all essential here. Each of such divisions of the roller will correspond with an angle upon the circle of 1° 24' 22,"5, or $\frac{1}{256}$th part of the circle. This number of principal divisions was chosen, on account of its being capable of continual bisec-

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* Sufficient spring for keeping the roller in close and uniform contact with the edge of the circle, is found in the apparatus, without any particular contrivance for that purpose; the bending of the pillars of the secondary frames and of the axis of the roller, chiefly supplies this property.

tion; but they do not fall in with the ultimate divisions of the circle, which are intended to be equal to 5′ each.

The next thing to be considered is, how to make the roller measure the circle. As two microscopes are here necessary, and those which I use are very simple, I will in this place give a description of them. Fig. 6 is a section of the full size, and sufficiently explains their construction, and the position of the glasses; but the micrometer part and manner of mounting it, are better shown at H, in Fig. 1 and 2. The micrometer part consists of an oblong square frame, which is soldered into a slit, cut at right angles in the main tube; another similar piece nicely fitted into the former, and having a small motion at right angles to the axis of the microscope, has at one end a cylindrical guide pin, and at the other a micrometer screw; a spring of steel wire is also applied, as seen in the section, to prevent play, by keeping the head of the micrometer in close contact with the fixed frame. This head is divided into one hundred parts, which are numbered each way to 50; the use of which will be shewn hereafter. A fine wire is stretched across the moveable frame, for the purpose of bisecting fine dots. Two of these microscopes are necessary; also a third, which need not have the divided head, and must have in the moveable frame two wires crossing each other at an angle of about 30°; this microscope is shewn at I, Fig. 1. In the two first micrometers, a division of the head is of the value of about 0″,2, and the power and distinctness such, that when great care is taken, a much greater error than to the amount of one of these divisions cannot well be committed in setting the wire across the image of a well made dot. The double eye-glass has a motion by hand, for producing distinct vision

of the wire; and distinct vision of the dots is procured by a similar adjustment of the whole microscope.

The first step towards sizing the roller, is to compute its diameter according to the measure of the circle, and to reduce it agreeably thereto, taking care to leave it a small matter too large. The second step is, after having brought the roller into its place in the Plate B B, to make a mark upon the surface of the circle near the edge, and a similar one upon the roller, exactly opposite each other ; then carry the apparatus forward with a steady hand, until the roller has made sixteen revolutions: If, now, the mark upon the roller, by having over-reached the one upon the circle, shews it to be much too large, take it out of the frame and reduce it by turning accordingly: When by repeating this, it is found to be very near, it may be turned about .001 of an inch smaller on the lower edge, and so far its preparation is completed. The third and last step is, the use and adaptation of the two microscopes ; one of these must take its position at H in Fig. 1, viewing a small well defined dot made for the purpose on the circle ; the other, not represented in the figure, must also be fixed to the main plate of Fig. 1, as near to the former as possible, but viewing one of the divisions on the roller. With a due attention to each microscope, it will now be seen to the greatest exactness when, by raising or depressing the roller, its commensurate diameter is found.

Fig. 3 is a representation of the apparatus for transferring the divisions of the roller to the circle. It consists of two slender bars, which, being seen edgewise in the figure, have only the appearance of narrow lines ; but, when looked at from above, they resemble the form of the letter A. They

are fastened to the main frame, as at W and Z, by short pillars, having also the off leg of the angle secured in the same manner; Y is a fine conical steel point for making the dots, and X is a feeler, whereby the point Y may be pressed down with a uniform force, which force may be adjusted, by bending the end of the bar just above the point, so as to make the dots of the proper size. The point Y yields most readily to a perpendicular action; but is amply secured against any eccentric or lateral deviation.

The apparatus, so far described, is complete for laying our foundation, *i. e.* making 256 primary dots; no matter whether with perfect truth, or not, as was said respecting the divisions of the roller; precision in either is not be expected, nor wished; but it is of some importance, that they should be all of the same size, concentric, small, and round. They should occupy a position very near the extreme border of the circle, as well to give them the greatest radius possible, as that there should be room for the stationary microscope and other mechanism, which will be described hereafter.

It must be noticed, that there is a clamp and adjusting screw attached to the main plate of Fig. 1; but, as it differs in no respect from the usual contrivances for quick and slow motion, it has been judged unnecessary to incumber the drawing with it.

Now, the roller having been adjusted, with one microscope H upon its proper dot on the circle, and the other microscope at the first division on the roller; place the apparatus of Fig. 3 so that the dotting point Y may stand directly over the place which is designed for the beginning of the divisions. In this position of things, let the feeler X be pressed down, until its

lower end comes in contact with the circle; this will carry down the point, and make the first impression, or primary dot, upon the circle; unclamp the apparatus, and carry it forwards by hand, until another division of the roller comes near the wire of the microscope; then clamp it, and with the screw motion make the coincidence complete; where again press upon the feeler for the second dot: proceed in this manner until the whole round is completed.

From these 256 erroneous divisions, by a certain course of examination, and by computation, to ascertain their absolute and individual errors, and to form these errors into convenient tables, is the next part of the process, and makes a very important branch of my method of dividing.

The apparatus must now be taken off, and the circle mounted in the same manner, that it will be in the Observatory. The two microscopes, which have divided heads, must also be firmly fixed to the support of the instrument, on opposite sides, and their wires brought to bisect the first dot, and the one which should be 180° distant. Now, the microscopes remaining fixed, turn the circle half round, or until the first microscope coincides with the opposite dot; and, if the other microscope be exactly at the other dot, it is obvious that these dots are 180° apart, or in the true diameter of the circle; and if they disagree, it is obvious that half the quantity by which they disagree, as measured by the divisions of the micrometer head, is the error of the opposite division; for the quantity measured is that by which the greater portion of the circle exceeds the less. It is convenient to note these errors + or —, as the dots are found too forward or too backward, according to the numbering of the degrees; and for the purpose

of distinguishing the $+$ and $-$ errors, the heads, as mentioned before, are numbered backwards and forwards to fifty. One of the microscopes remaining as before, remove the other to a position at right angles; and, considering for the present both the former dots to be true, examine the others by them; *i. e.* as before, try by the micrometer how many divisions of the head the greater half of the semi-circle exceeds the less, and note half the quantity $+$ or $-$, as before, and do the same for the other semi-circle. One of the micrometers must now be set at an angle of $45°$ with the other, and the half differences of the two parts of each of the four quadrants registered with their respective signs. When the circle is a vertical one, as in the present instance, it is much the best to proceed so far in the examination with it in that position, for fear of any general bending or spring of the figure; but, for the examination of smaller arcs than $45°$, it will be perfectly safe, and more convenient, to have it horizontal; because the dividing apparatus will then carry the micrometers, several perforations being made in the plate B for the limb to be seen through at proper intervals. The micrometers must now be placed at a distance of $22° 30'$, and the half differences of the parts of all the arcs of $45°$ measured and noted as before; thus descending by bisections to $11° 15'$, $5° 37' 30''$, and $2° 48' 45''$. Half this last quantity is too small to allow the micrometers to be brought near enough; but it will have the desired effect, if they are placed at that quantity and its half, *i. e.* $4° 13' 7'',5$; in which case the examination, instead of being made at the next, will take place at the next division but one, to that which is the subject of trial. During the whole of the time that the examination is made, all the dots, except the

one under examination, are for the present supposed to be in their true places; and the only thing in this most important part of the business, from first to last, is to ascertain with the utmost care, in divisions of the micrometer head, how much one of the parts of the interval under examination exceeds the other, and carefully to tabulate the half of their difference.

I will suppose that every one, who attempts to divide a large astronomical instrument, will have it engraved first. Dividing is a most delicate operation, and every coarser one should precede it. Besides, its being numbered is particularly useful to distinguish one dot from another; thus, in the two annexed tables of errors, the side columns give significant names to every dot, in terms of its value to the nearest tenth of a degree, and the mistaking of one for another is rendered nearly impossible.

The foregoing examination furnishes materials for the construction of the table of half differences, or apparent errors.* The first line of this table consists of two varieties; *i. e.* the micrometers were at 180° distance for obtaining the numbers which fill the columns of the first and third quadrant; and at 90°, for those of the second and fourth quadrant. The third variety makes one line, and was obtained with a distance of 45°: the fourth consists of two lines, with a distance of 22° 30′: the fifth of four lines, with a distance of 11° 15′: the sixth of eight lines, with a distance of 5° 37′ 30″: the seventh of sixteen lines, with a distance of 2° 48′ 45″: and the eighth and

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* If the table of real errors be computed as the work of examination proceeds, there will be no occasion for this table at all; but, I think it best not to let one part interfere with another, and therefore I examine the whole before I begin to compute.

last variety, being the remainder of the table, consists of thirty two lines, and was obtained with a distance of 4° 13′ 7″,5.

The table of apparent errors, or half differences, just explained, furnishes data for computing the table of real errors. The rule is this; let a be the real error of the preceding dot, and b that of the following one, and c the apparent error, taken from the table of half differences, of the dot under investigation; then is $\frac{a+b}{2} + c =$ its real error. But, as this simple expression may not be so generally understood by workmen as I wish, it may be necessary to say the same thing less concisely. If the real errors of the preceding and following dots are both $+$, or both $-$, take half their sum and prefix thereto the common sign ; but, if one of them is $+$, and the other $-$, take half their difference, prefixing the sign of the greater quantity : again, if the apparent error of the dot under investigation has the same sign of the quantity found above, give to their sum the common sign, for the real error; but if their signs are contrary, give to their difference the sign of the greater for the real error. I add a few examples.

Example 1.

For the first point of the second quadrant.

Real error of the first point of the first quadrant	$-$ 0,0
Real error of the first point of the third quadrant	$-$ 6,9
Half sum or half difference	$-$ 3,4
Apparent error of the dot under trial	$+$ 12,2
Real error	$+$ 8,8

Example 2.

For the point 45° of the second quadrant.

Real error of the first point of the quadrant	-	+ 8,8
Real error of the last point of the quadrant	-	— 6,9
Half difference - - - -	+ 0,9
Apparent error of the dot under trial -	— 8,9
Real error - - - - -	— 8,0

Example 3.

Point 88°,6, or last point, of the third quadrant.

Real error of the point 84°,4 of the third quadrant	— 21,0
Real error of the point 2°,8 of the fourth quadrant	— 2,9
Half sum - - - - -	— 11,9
Apparent error of the dot under trial -	— 4,0
Real error - - - - -	— 15,9

Example 4.

Point 88°,6, or last, of the fourth quadrant.

Real error of the point 84°,4 of the fourth quadrant	— 21,6
Real error of the point 2°,8 of the first quadrant -	— 10,2
Half sum - - - - -	— 15,9
Apparent error of the dot under trial -	+ 9,5
Real error - - - - -	— 6,4

It is convenient, in the formation of the table of real errors, that they should be inserted in the order of the numbering of the degrees on their respective quadrants; although their computation necessarily took place in the order in which the examination was carried on, or according to the arrangement in the table of apparent errors. The first dot of the first

quadrant having been assumed to be in its true place, the first of the third quadrant will err by just half the difference found by the examination; therefore these errors are alike in both tables. The real error of the first dot of the second quadrant comes out in the first example; that of the fourth was found in like manner, and completes the first line. It is convenient to put the error of the division 90° of each quadrant at the bottom of each column, although it is the same as the point 0° on the following quadrant. The line of 45° is next filled up; the second example shews this; but there is no occasion to dwell longer upon this explanation; for every one, who is at all fit for such pursuits, will think what has already been said fully sufficient for his purpose. However, I will just mention that there can be no danger, in the formation of this table, of taking from a wrong line the real errors which are to be the criterion for finding that of the one under trial; because they are in the line next to it; the others, which intervene in the full table, not being yet inserted. The last course of all is, however, an exception; for, as the examining microscopes could not be brought near enough to bisect the angle 2° 48′ 45″, recourse was had to that quantity and its half; on which account the examination is prosecuted by using errors at two lines distance, as is shewn in the two last examples.

When the table of real errors is constructed, the other table, although it is of no further use, should not be thrown away; for, if any material mistake has been committed, it will be discovered as the operation of dividing is carried on; and, in that case, the table of apparent errors must be had recourse to; indeed, not a figure should be destroyed until the work is done.

Respecting the angular value of the numbers in these tables, it may be worth mentioning, that it is not of the least importance; 100 of them being comprised in one revolution of the micrometer screw; and, in the instance before me, 5,6 of them made no more than a second. It is not pretended that one of these parts was seen beyond a doubt, being scarcely $\frac{1}{50000}$ of an inch, much less the tenths, as exhibited in the tables; but, as they were visible upon the micrometer heads, it was judged best to take them into the account.

Having now completed the two first sections of my method of dividing; namely, the first, which consists of making 256 small round dots; and the second, in finding the errors of those dots, and forming them into a table; I come now to the third and last part, which consists in using the erroneous dots in comparison with the tabulated errors, so as ultimately to make from them the true divisions.

It will here be necessary to complete the description of the remaining part of the apparatus. And first, a little instrument which I denominate a subdividing sector presents itself to notice. From all that has hitherto been said, it must have been supposed that the roller itself will point out, upon the limb of the instrument to be divided, spaces corresponding to others previously divided upon itself, as was done in setting off the 256 points: but, to obviate the difficulty of dividing the roller with sufficient exactness, recourse was had to this sector; which also serves the equally important purpose of reducing the bisectional points to the usual division of the circle. This sector is represented in full dimensions by Fig. 5: it is formed of thin brass, and centred upon the axis at A, in contact with the upper surface of the roller: it is capable of

being moved round by hand; but, by its friction upon the axis and its pressure upon the roller, it is sufficiently prevented from being disturbed by accident. An internal frame B B, to which the arc C C is attached, moves freely in the outer one, and by a spring D is pushed outwards, while the screw E, whose point touches the frame B, confines the arc to its proper radius. The arc of this sector is of about four times greater radius than the roller, and upon it are divided the spaces which must be transferred to the instrument, as represented on a magnified scale by Fig. 4. Now, the angle of one of the spaces of the circle will be measured by sixteen times its angular value upon the sectorial arc, or 22° 30′; but this does not represent any number of equal parts upon the instrument, whose subdivisions are to be 5′ each; for $\frac{1° \, 24' \, 22''5}{5}$ is exactly $16\frac{7}{8}$, therefore so many divisions are exactly equal to a mean space between the dots whose errors have been tabulated. Let, therefore, the arc of the sector be divided into 16 spaces of 1° 20′ each, and let a similar space at each end be subdivided into eight parts of 10′ each, as in Fig. 4; we shall then have a scale which furnishes the means for making the true divisions, and an immediate examination at every bisectional point.

I have always divided the sector from the engine, because that is the readiest method, and inferior to none in point of accuracy, where the radius is very short; but, as it is more liable than any other to centrical error, the adjustment of the arc by the screw E becomes necessary: by that adjustment, also, any undue run in the action of the roller may be reduced to an insensible quantity.*

* See note page 130.

When the utmost degree of accuracy is required, I give the preference to dividing by lines, because they are made with a less forcible effort than dots are ; and also because, if any small defect in the contexture of the metal causes the cutter to deviate, it will, after passing the defective part, proceed again in its proper course, and a partial crookedness in the line will be the only consequence ; whereas a dot, under similar circumstances, would be altogether displaced. But, on the other hand, where accuracy has been out of the question, and only neatness required, I have used dots ; and I have done so, because I know that when a dot and the wire which is to bisect it are in due proportion to each other, (the wire covering about two thirds of the dot) the nicest comparison possible may be obtained. It may be further observed, that division by lines is complete in itself ; whereas that by dots requires lines to distinguish their value.

On the upper side of Fig. 1 is represented the apparatus for cutting the divisions. It consists of three pieces J K L, jointed together so as to give to the cutter an easy motion for drawing lines directly radiating from the centre, but inflexible with respect to lateral pressure ; dd are its handles. The cutting point is hidden below the microscope H ; it is of a conical form, and were it used as a dotting point, it would make a puncture of an elliptical shape, whose longer diameter would point towards the centre. This beautiful contrivance, now well known, we owe to the ingenuity of the late Mr. Hindley of York ; it was borrowed by Mr. Ramsden,[*] and applied with the best effect to his dividing engine.

[*] This I learned from that most accurate artist Mr. John Stancliffe, who was himself apprentice to Hindley.

It might have been mentioned sooner, that in the instance which I have selected as an example of my dividing, the operation took place when the season of the year, and the smoke of London, had reduced the day to scarcely six hours of effective light; and rather than confine my labours within such narrow limits, I determined to shut out the day-light altogether. Fig. 7 shews the construction of the lanterns which I used. A very small wick gave sufficient light, when kept from diverging by a convex lens; while the inclining nossel was directed down exactly upon the part looked at, and the light, having also passed through a thin slice of ivory, was divested of all glare. I enter into this description, because, I think, I never saw my work better, nor entirely to so much advantage as in this instance; owing, perhaps, to the surrounding darkness allowing the pupil of the eye to keep itself more expanded, than when indirect rays are suffered to enter it. The heat from a pair of these lanterns was very inconsiderable, and chiefly conducted along with the smoke up the reclining chimney.

Previous to cutting the divisions, the parts now described must be adjusted. The cutting apparatus must be placed with the dividing point exactly at the place where the first line is intended to be drawn, and clamped, so that the adjusting screw may be able to run it through a whole interval. The microscope H must be firmly fixed by its two pillars *b b* to the main frame, with its micrometer head at *zero;* and with its only wire in the line of the radius, bisecting the first of the 256 dots. And it should be observed, that the cutting frame and this must not vary respecting each other, during the time that the divisions are cut; for any motion that took place in either

would go undiminished to the account of error. The micro-
scope I is also fastened to the main frame; but it is only re-
quired to keep its position unvaried, while the divisions of the
sector pass once under its notice; for it must have its wires
adjusted afresh to these divisions at every distinct course
The microscope I has two wires, crossing each other at an
angle of about 40°; and these are to be placed so as to make
equal angles with the divisions of the sector, which are not
dots, but lines. The sectorial arc must also be adjusted to its
proper radius by the screw E, Fig. 5; *i. e.* while the main
frame has been carried along the circle through a mean in-
terval shewn by H, the sector must have moved through
exactly $16\frac{7}{8}$ of its divisions, as indicated by I.*

Things being in this position; after having given the parts
time to settle, and having also sufficiently proved the perma-
nence of the micrometer H and the cutting frame with respect
to each other, the first division may be made; then, by means
of the screw for slow motion, carry the apparatus forward, until
the next line upon the sector comes to the cross wires of I; you
then cut another division, and thus proceed until the 16th divi-
sion is cut, $= 1° 20'$: Now the apparatus wants to be carried

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* For the sake of simplicity, the account of the process is carried on as if the roller
measured the mean interval without error: But it was said (Page 107) that the roller,
in a continued motion quite round the circle, would in some part of its course err by
30″ or more; therefore, when that is the case, an extreme run of the roller cannot
agree with a mean interval of the circle nearer than $\frac{30''}{128} = 0'',23$; and most probably
this kind of error will on some intervals amount to double that quantity. It there-
fore becomes matter of prudent precaution to examine every interval previous to
making the divisions; and, where necessary, to adjust the sector, so that its arc may
exactly measure the corresponding interval as corrected by the tabulated errors.

further, to the amount of $\frac{7}{8}$ of a division, before an interval is complete; but at this last point no division is to be made; we are here only to compare the division on the sector with the corresponding dot upon the instrument: This interval, however, upon the circle will not be exactly measured by the corresponding line of the sector, which has been adjusted to the mean interval, for the situation of the dot 1°,4 is too far back, as appears by the table of real errors, by — 4,8 divisions of the micrometer head. The range of the screw for slow motion must now be restored, the cross wires of H set back to — 4,8 divisions, and the sector moved back by hand, but not to the division o where it began before; for, as it left off in the first interval at $\frac{7}{8}$ of a division, it has to go forwards $\frac{1}{8}$ more before it will arrive at the spot where the 17th division of the instrument 1° 25′ is to be made, so that in this second course it must begin at $\frac{1}{8}$ short of o : Go through this interval as before, making a division upon the circle at every one of the 16 great divisions of the sector; and H should now reach the third dot, allowing for a tabular error of — 10,2 when the division $\frac{6}{8}$ths of the sector reaches the cross wires of I. It would be tedious to lead the reader through all the variety of the sector, which consists of eight courses; and it may be sufficient to observe, that at the commencement of every course, it must be put back to the same fraction of a division which terminated its former one; and that the wire of the micrometer H must always be set to the tabular error belonging to every dot, when we end one interval and begin another. The eight courses of the sector will have carried us through $\frac{1}{32}$ part of the circle, 11° 15′, and during this time, the roller will have proceeded through half a revolution; for

its close contact with the limb of the circle does not allow it to return with the sector when the latter is set back at every course. Having in this manner proceeded, from one interval to another, through the whole circle, the micrometer at last will be found with its wire, at *zero*, on the dot from which it set out; and the sector, with its 16th division, coinciding with the wires of its microscope.

Having now given a faithful detail of every part of the process of dividing this circle, I wish to remind the reader that, by verification and correction at every interval, any erroneous action of the roller is prevented from extending its influence to any distant interval. It will be further observed, that the subdividing sector magnifies the work; that by means of its adjustable arc, it makes the run of the roller measure its corresponding intervals upon the circle; and, without foreign aid, furnishes the means of reducing the bisectional intervals to the usual division of the circle. Furthermore, the motion of the wire of the micrometer H, according to the divisions of its head and corresponding table of errors, furnishes the means of prosecuting the work with nearly the same certainty of success, as could have happened, had the 256 points been (which in practice is quite impossible) in their true places.

Now, the whole of my method of dividing being performed by taking short measures with instruments which cannot themselves err in any sensible degree, and, inasmuch as those measures are taken, not by the hand, but by vision, and the whole performed by only looking at the work, the eye must be charged with all the errors that are committed until we come to cut the divisions; and, as in this last operation the

hand has no more to do than to guide an apparatus so perfect in itself, that it cannot be easily made to deviate from its proper course, I would wish to distinguish it from the other methods by denominating it, DIVIDING BY THE EYE.[*]

The number of persons at all capable of dividing originally have hitherto been very few; the practice of it being so limited, that, in less than twice seven years, a man could hardly hope to become a workman in this most difficult art. How far I shall be considered as having surmounted these difficulties, I know not; but if, by the method here revealed,

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[*] I must here remark, that SMEATON has represented the greatest degree of accuracy that can be derived from vision, in judging of the coincidence of two lines at $\frac{1}{4000}$ part of an inch. From this it may fairly be inferred, that he had not cultivated the power of the sight, as he had done that of the touch; the latter of which, with that ability which appeared in all his works, he rendered sensible to the $\frac{1}{60000}$ part of an inch. Were materials infinitely hard, no bounds could be set to the precision of contact; but taking things as they are, the different degrees of hardness in matter, may be considered as a kind of magnifying power to the touch, which may not unaptly be compared with the assistance which the eye receives from glasses. It is now quite common to divide the seaman's sextant to 10″, and a good eye will estimate the half of it; which, on an eight inch radius is scarcely $\frac{1}{10000}$ of an inch. This quantity, small as it is, is rendered visible by a glass of one inch focal length; and such is the certainty with which these quantities are seen, that a seaman will sometimes complain that two pair of these lines will coincide at the same time; and that may happen, and yet no division of his instrument err, by more than $\frac{1}{20000}$ part of an inch. All this is applicable to judging of the coincidence of *lines* with each other, and furnishes not the most favourable display of the accuracy of vision. But with the microscopes here described, where the wire bisects the image of a dot, or a cross wire is made to intersect the image of a line, by an eye practised in such matters, a coincidence may undoubtedly be ascertained to $\frac{1}{50000}$ part of an inch. I am of opinion that as small a quantity may be rendered visible to the eye, as can by contact be made sensible to the touch; but whether Mr. SMEATON's $\frac{1}{60000}$ and my $\frac{1}{30000}$ be not the same thing, I will not determine; the difference between them, however, is what he would no more have pretended to feel, than I dare pretend to see.

I have not rendered original dividing almost equally easy with what copying was before, I have spent much labour, time, and thought in vain. I have no doubt indeed, that any careful workman who can divide in common, and has the ability to construct an astronomical instrument, will, by following the steps here marked out, be able to divide it, the first time he tries, better than the most experienced workman, by any former method.

If, instead of subdividing with the roller, the same thing be performed with the screw, it will not give to dividing by the eye any very distinctive character: I have practised this on arcs of circles with success, the edge being slightly racked, the screw carrying forward an index with the requisite apparatus, and having a divided micrometer head; the latter answers to the subdividing sector, and, being used with a corresponding table of errors, forms the means of correcting the primitive points; but the roller furnishes a more delicate action, and is by far more satisfactory and expeditious.

It is known to many that the six feet circle, which I am now at work upon for our Royal Observatory, is to be divided upon a broad edge, or upon a surface at right angles to the usual plane of division: The only alterations, which will on that account be required, are, that the roller must act upon that plane which is usually divided upon; which roller, being elevated or depressed, may be adjusted to the commensurate radius without being made conical, as was necessary in the other case. The apparatus, similar to the other, must here be fixed immoveable to the frame which supports the circle; its position must be at the vertex, where also I must have my station; and the instrument itself must be turned around its axis, in its

proper vertical position, as the work proceeds. The above may suffice, for the present, to gratify those who feel themselves interested upon a subject which will be better understood, if I should hereafter have the honour of laying before the Royal Society a particular description of the instrument here alluded to ; a task which I mean to undertake, when, after being fixed in the place designed for it, which I hope will be effected at no very distant period, it shall be found completely to answer the purposes intended.

Should it be required to divide a circle according to the centesimal division of the quadrant, as now recommended and used in France, we shall have no difficulty. The 100° of the quadrant may be conveniently subdivided into 10 each, making 4000 divisions in the whole round. The 256 bisectional intervals, the two tables of errors, and the manner of proceeding and acting upon them will be exactly the same as before, until we come to cut the divisions ; and for this purpose we must have another line divided upon the sector. For $\frac{1}{4000}$ part of the circle being equal to $5',4$ of the usual angular measure $\frac{1° 24' 22'',5}{5',4} = 15\frac{5}{8}$ divisions ; and just so many will be equivalent to one of the intervals of the circle. The value of one of the great divisions of the sector will be $1° 26' 24''$, and that of the $\frac{1}{8}$ parts, which are to be annexed to the right and left as before, will be $10' 48''$, therefore divisible by the engine. Should any astronomer choose to have both graduations upon his instrument, the additional cost would be a mere trifle, provided both were done at the same time.

It must already have been anticipated, that dividing by the eye is equally applicable to straight lines as it is to circles.

An apparatus for this purpose should consist of a bar of brass, three quarters of an inch thick, and not less than three inches broad; six feet may do very well for the length; it may be laid upon a deal plank strengthened by another plank screwed edge-wise on its lower surface. The bar should be planed, on both its edges and on its surface, with the greatest exactness; and it will be better, if it has a narrow slip of silver, inlaid through its whole length, for receiving the dots. An apparatus nearly similar to the other should slide along its surface, carrying a roller, whose circumference is 12,8 inches, and turned a little conical for the sake of adjustment. The roller may be divided into 32 parts, each of which when transferred to the bar will give intervals of 0,4 of an inch each: The angle of the sub-dividing sector should of course be 11° 15′, and subdivided into four parts, which will divide the inch into tenths: The surface may also receive other lines, with subdivisions suited to the different purposes for which it may be wanted. The revolutions of the roller and its $\frac{1}{32}$ parts must be dotted upon the bar; taking care, by sizing the roller, to come as near the true standard measure as possible: When this is done, compare the extent of the greater bisectional number that is contained in the length; *i. e.* 128 intervals or 51,2 inches, with the standard measure; noting the difference as indicated by the micrometer heads: The examination and construction of the table of errors may then be conducted just as was done for the circle.

Being now ready for the performance of its work, the scale to be divided must be laid alongside of the bar, and the true divisions must be cut upon it by an appeal, as before, to the erroneous dots on the bar, corrected by a corresponding table

of errors. The apparatus, remaining entire in the possession of the workman, with its primitive dots, the table of errors, &c. is ready for dividing another standard, which will be precisely similar to others that have been, or may be, divided from it. It may be considered, indeed, as a kind of engine; and, as it is not vitiated by the coarse operation of racking with a screw, but performed by only looking at the work, the method will command about three times the accuracy that can be derived from the usual straight-line dividing engine. Should it be asked, if an engine thus appointed would succeed for dividing circles? I answer, Yes; but I would not recommend it; because, beyond a certain extent of radius, it is not necessary; for the errors, which would be introduced into the work by the violence of racking a large wheel, are sufficiently reduced by the comparative shortness of the radius of such instruments as we divide by that method: And, what is still more to the purpose, the dividing engine is four times more expeditious, and bears rough usage better. I cannot quit the subject of dividing straight lines without observing, that I never had my apparatus complete. The standard which I made for Sir GEORGE SHUCKBURG EVELYN in 1796 was done by a mere make-shift contrivance, upon the principle of dividing by the eye; how I succeeded may be seen in Sir GEORGE's papers on Weights and Measures (Phil. Trans. for 1798). I made a second, some years after, for Professor PICTET of Geneva, which became the subject of comparison with the new measure of France, before the National Institute; and their report, drawn up by Mr. PICTET, has been ably re-stated and corrected by Dr. YOUNG, as published in the Journals of the Royal Institution. I made a third for the Magistrates of

Aberdeen. I notice the two latter, principally to give myself an opportunity of saying that, if those three scales were to be compared together, notwithstanding they were divided at distant periods of time, and at different seasons of the year, they would be found to agree with each other, as nearly as the different parts of the same scale agree.

I hope I may here be allowed to allude to an inadvertency which has been committed in the paper mentioned above; and which Sir GEORGE intended to have corrected, had he lived to conclude his useful endeavours to harmonize the discordant weights and measures of this country. The instruments which he has brought into comparison are, his own five feet standard measure and equatorial; General ROY's forty-two inch scale; the standard of Mr. AUBERT; and that of the Royal Society. The inadvertency is this: In his equatorial, and the standard of the Royal Society, he has charged the error of the most erroneous extent, when compared with the mean extent, alike to both divisions; *i. e.* he has supposed one of the divisions, which bound the erroneous extent, to be too much to the right, and the other too much to the left, and that by equal quantities: This is certainly a good natured way of stating the errors of work; and perhaps not unjustly so, where the worst part has been selected; but, in the other three instances, namely, in General ROY's, Mr. AUBERT's, and his own standard, he has charged the whole error of the most erroneous extent to one of the bounding lines.

I was well confirmed in my high opinion of the general accuracy of BIRD's dividing, when, last winter,[*] I measured the chords of many arcs of the Greenwich quadrant: That instrument has indeed suffered both from a change in its figure,

* This paper was written in June 1808.

and from the wearing of its centre; but the graduation, con-
sidering the time when it was done, I found to be very good.
Sir GEORGE in his Paper upon the Equatorial (Phil. Trans. for
1793), after some compliments paid to the divider of his in-
strument, says, " the late Mr. JOHN BIRD seems to have ad-
" mitted a probable discrepancy in the divisions of his eight
" feet quadrant amounting to $3''$;" and he refers to BIRD on
the construction of the Greenwich quadrant. This quantity
being three times as great as any errors that I met with, I
was lately induced to inquire how the matter stood. BIRD, in
the paper referred to, says, " in dividing this instrument I
" never met with an inequality that exceeded one second. I
" will suppose that in the 90 arch this error lay towards the
" left hand, and in the 96 arch that it lay towards the right,
" it will cause a difference between the two arches of two
" seconds; and, if an error of one second be allowed to the
" observer in reading off his observation, the whole amount
" is no more than three seconds, which is agreeable to what
" I have heard, &c." Sir GEORGE's examination of his own
Equatorial furnishes me with the means of a direct compari-
son: In his account of the declination circle, we find an error
$+ 2'',35$, and another $- 1'',5$; to these add an error of half
a second in each, for reading off, which Sir GEORGE also ad-
mits, we shall then have a discrepancy of $4'',85$; but, as the
errors of reading off are not errors of division, let them be
discharged from both, and the errors will then stand, for the
quadrant $2''$, and for the circle $3'',85$. As the radius of the
former, however, is four times greater than that of the latter,
it will appear, by this mode of trial, that the Equatorial is
rather more than twice as accurately divided as the quadrant.

In doing justice to Bird in this instance, I have only done as I would be done by; for, should any future writer set me back a century on the chronological scale of progressive improvement, I hope some one will be found to restore me to my proper niche. I now subjoin a re-statement of the greatest error of each of the instruments that are brought into comparison by Sir George, after having reduced them all by one rule; viz. allowing each of the two points which bound the most erroneous extent to divide the apparent error equally between them. They are expressed in parts of an inch, and follow each other in the order of their accuracy.

Sir George Shuckburg's 5 feet standard	,000165
General Roy's scale of 42 inches - -	,000240
Sir George's Equatorial, 2 feet radius -	,000273
The Greenwich quadrant, 8 feet radius -	,000465
Mr. Aubert's standard, 5 feet long -	,000700
* The Royal Society's standard 92 inches long	,000795

For the justness of the above statement I consider my name as pledged; requesting the permission to say, that if on the result of each respective examination, as here presented, there could have been more than one opinion, it would not have appeared here. I am further prompted to add, that the above comparative view presents one circumstance to our notice, which cannot do less than gratify every individual who is at all conversant in these matters; I mean, the high rank which General Roy's scale takes in the list; that scale having been made the agent in measuring the base line of our national trigonometrical survey.

* This is the same which Mr. Bird used in dividing his eight feet mural quadrants, and was presented to the Royal Society by Bird's executors.

To return, finally, to the dividing of circles; I must state, as matter of precaution, that great care should be taken during the turning of the outer edge, to have the circle of the same temperature; for one part may be expanded by heat, or contracted by cold, so much more than another, as to cause the numbers in the tables of errors to be inconveniently large. A night is not more than sufficient for allowing the whole to take the same temperature, after having been handled by the workmen; and the finishing touch should be given within a short space of time. But, if the effects of temperature are to be regarded in turning a circle, it is of tenfold more importance to attend to this circumstance, while the examination of the larger arcs of the instrument is carried on; for it is absolutely necessary that, during this time, the whole circle should be of the same heat exactly. Few workmen are sufficiently aware of this: They generally suppose the expansion of metals to be a trifle which need not be regarded in practice; and wonder how the parts of a circle can be differently heated without taking pains to make it so. One degree of Fahrenheit's thermometer indicates so small a portion of heat that, in such places as workmen are usually obliged to do their business in, it is not very easy to have three thermometers attached to different parts of a large instrument, shewing an equality of temperature within that quantity: Yet so necessary is correctness in this respect, that if a circle has the vertex one degree warmer than its opposite, and if this difference of temperature be regularly distributed from top to bottom, the upper semi-circle will actually exceed the lower by 2″: And, if such should happen to be the case while the examination of the first dot of the third quadrant is made,

the regularity of the whole operation would thereby be destroyed.

It may not be improper to remark, that dividing by the eye does not require a more expensive apparatus than the operation of dividing by hand; and, indeed, less so when the scale of inches is deemed necessary. The method by adjustment is still more expensive, requiring whatever tools BIRD's method requires, and, in addition to these, a frame and microscopes, somewhat similar to those for dividing by the eye.

It is somewhat more difficult to give a comparative estimate of the time which the different methods of dividing require. I know that thirteen days of eight hours each, are well employed in dividing such a circle by my method; about fifty-two days would be consumed in doing the same thing by BIRD's method; and I think I cannot err much when I state the method by adjustment, supposing every dot to be tried, and that two-thirds of them want adjusting, to require about one hundred and fifty of such days.

The economy of time (setting aside the decided means of accuracy) which the above estimate of its application offers to view, will, I think, be considered of no little moment. By the rising artist who may aspire at excellence, it will at least, and I should hope, with gratitude, be felt in the abbreviation of his labours. To me, indeed, the means of effecting this became indispensible; and it has not been without a sufficient sense of its necessity, that I have been urged to the progressive improvement and completion of these means, as now described. It is but little that a man can perform with his own hands alone; nor is it on all occasions, even in frames of firmer texture than my own, that he can decisively command their

adequate, unerring, use. And I must confess that I never could reconcile it to what I hold as due to myself, as well as to a solicitous regard for the most accurate cultivation of the science of astronomy, to commit to others an operation requiring such various and delicate attentions, as the division of my instruments.

That my attentions on this head have not failed to procure for me the notice and patronage of men whose approbation makes, with me, no inconsiderable part of my reward, I have to reflect on with gratitude and pleasure: And as I look with confidence to the continuance of that patronage so long as the powers of execution shall give me the inclination to solicit it, I cannot entertain a motive which might go to extinguish the more liberal wish of pointing out to future ingenuity a shorter road to eminence; sufficiently gratified by the idea of having in the present communication, contributed to facilitate the operations, and to aid the progress of art (as far as the limited powers of vision will admit) towards the point of perfection.

Table of apparent Errors.

Name of the Dot	First Quadrant	Second Quadrant	Third Quadrant	Fourth Quadrant	First Quadrant	Second Quadrant	Third Quadrant	Fourth Quadrant	Name of the Dot
°	°								°
0,0	0	+12,2	−6,9	+17,9	+4,6	−17,1	−4,4	+17,3	1,4
45,0	−21,3	8,9	16,7	−29,6	5,2	9,7	8,9	6,4	4,2
22,5	1,6	2,2	1,0	2,7	0,0	3,8	1,0	4,7	7,0
67,5	+1,0	+15,6	0,0	+13,7	+1,0	3,5	5,1	5,5	9,8
11,2	−16,6	−20,2	22,6	−30,3	−5,5	−1,6	0,0	+1,2	12,7
33,7	4,0	4,2	13,2	23,1	7,6	7,6	4,2	2,3	15,5
56,2	16,9	22,2	17,0	22,7	9,4	3,9	0,0	5,3	18,3
78,7	30,8	16,6	31,3	30,3	+1,1	+12,1	+4,2	4,3	21,1
5,6	2,7	8,6	4,1	10,1	12,3	0,9	6,2	14,4	23,9
16,9	11,5	11,3	11,2	16,1	5,7	6,2	1,1	−11,2	26,7
28,1	9,0	7,4	5,8	14,3	+1,5	3,5	6,3	+4,2	29,5
39,4	9,3	8,2	5,8	13,1	0,8	7,9	7,7	1,4	32,3
50,6	4,2	6,6	8,2	4,4	1,5	9,0	3,0	4,3	35,2
61,9	4,3	8,4	12,5	4,4	8,6	5,9	2,0	6,7	38,0
73,1	7,6	10,0	13,6	9,7	3,3	2,7	4,9	1,5	40,8
84,4	18,0	+6,0	16,3	7,1	4,0	3,1	3,5	+1,0	43,6
2,8	3,4	−7,5	8,9	2,1	+13,5	10,5	+16,0	14,9	46,4
8,4	0,0	5,0	4,6	5,7	2,1	0,0	1,7	3,5	49,2
14,1	6,6	8,2	5,6	4,8	−5,0	−10,7	2,9	1,5	52,0
19,7	1,6	2,4	1,0	2,5	4,2	7,9	2,2	7,2	54,8
25,3	3,7	+8,2	−2,9	2,5	4,0	3,0	2,5	1,0	57,7
30,9	2,4	7,1	7,9	0,0	7,3	6,2	6,1	1,5	60,5
36,6	−5,9	+1,0	2,5	1,5	−3,2	−10,1	5,6	12,7	63,6
42,2	+3,1	1,9	5,8	2,5	1,4	7,2	3,9	+2,2	66,1
47,8	7,1	5,2	+2,4	+4,8	+11,2	+14,9	+21,2	7,2	68,9
53,4	−5,6	−6,0	5,0	−6,1	7,1	1,0	8,9	−11,7	71,1
59,1	10,7	+1,0	3,0	1,4	5,3	1,2	6,6	2,7	74,5
64,7	7,9	−18,0	10,7	−9,0	7,2	9,9	+1,0	5,9	77,3
70,3	2,7	7,4	1,5	9,0	6,5	1,8	5,3	2,6	80,2
75,9	1,2	5,2	2,2	4,7	4,4	1,4	2,2	4,3	83,0
81,6	1,6	+1,7	0,0	2,0	+20,8	0,0	11,4	+1,0	85,8
87,2	13,7	+6,0	3,5	+5,6	+2,1	+11,0	4,0	9,5	88,6

Mr. Troughton on dividing Instruments.

Fig 1

Fig. 3

Fig. 4

A B C D G H

8 9 10 11 12 13

Fig 5

Fig 6

Fig. 7
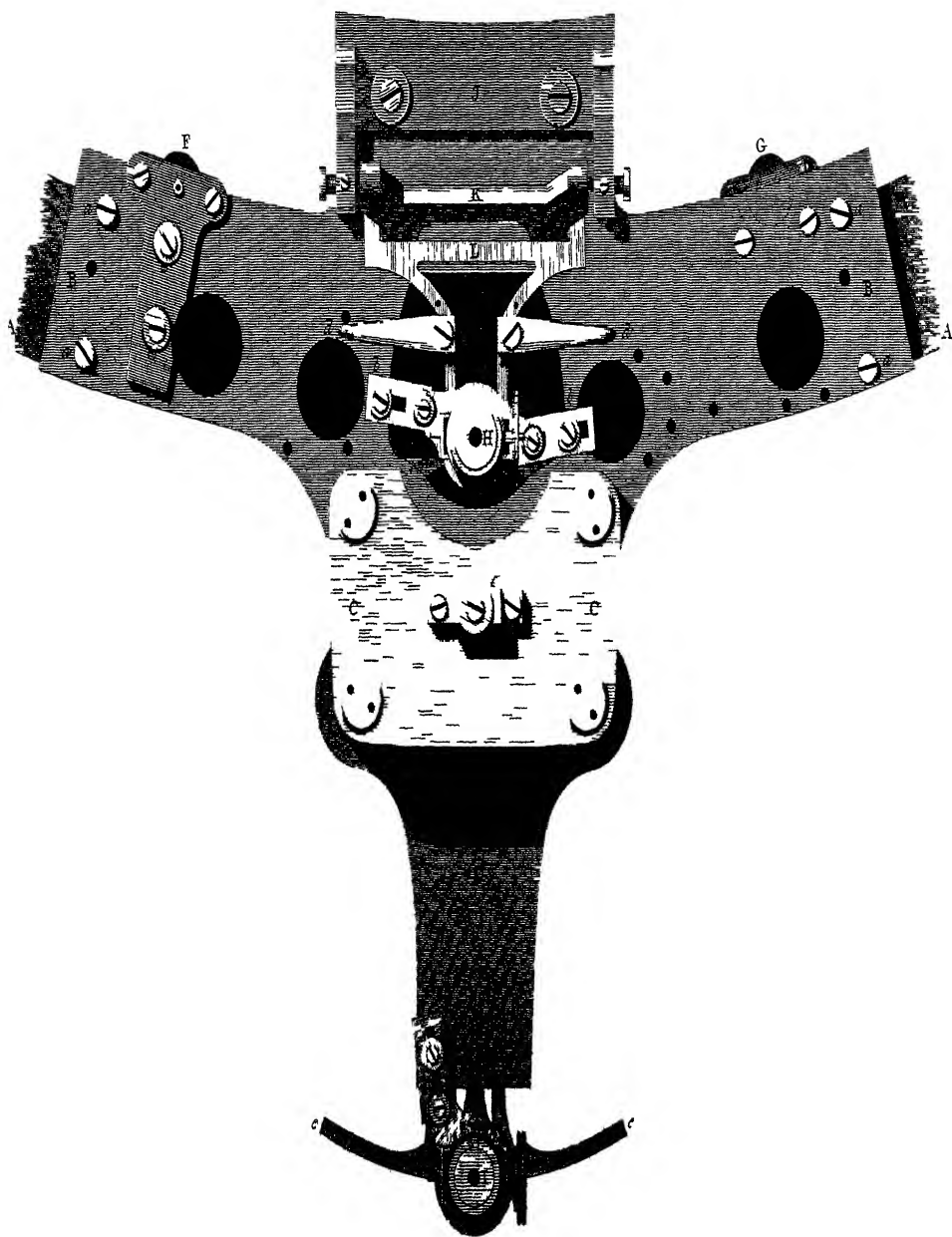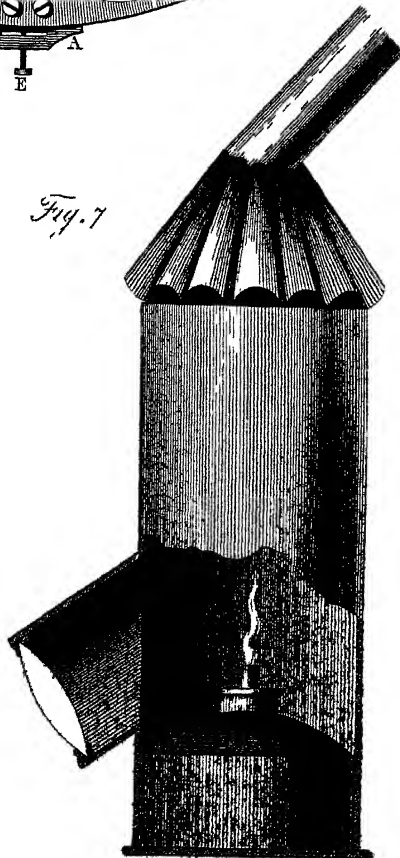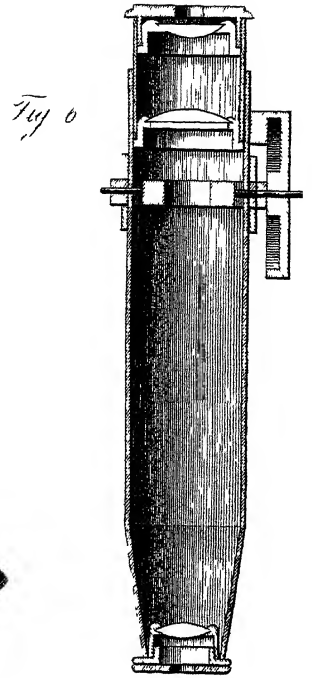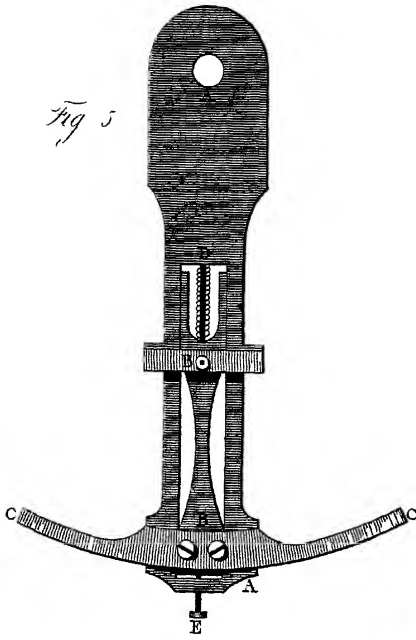
Table of real Errors.

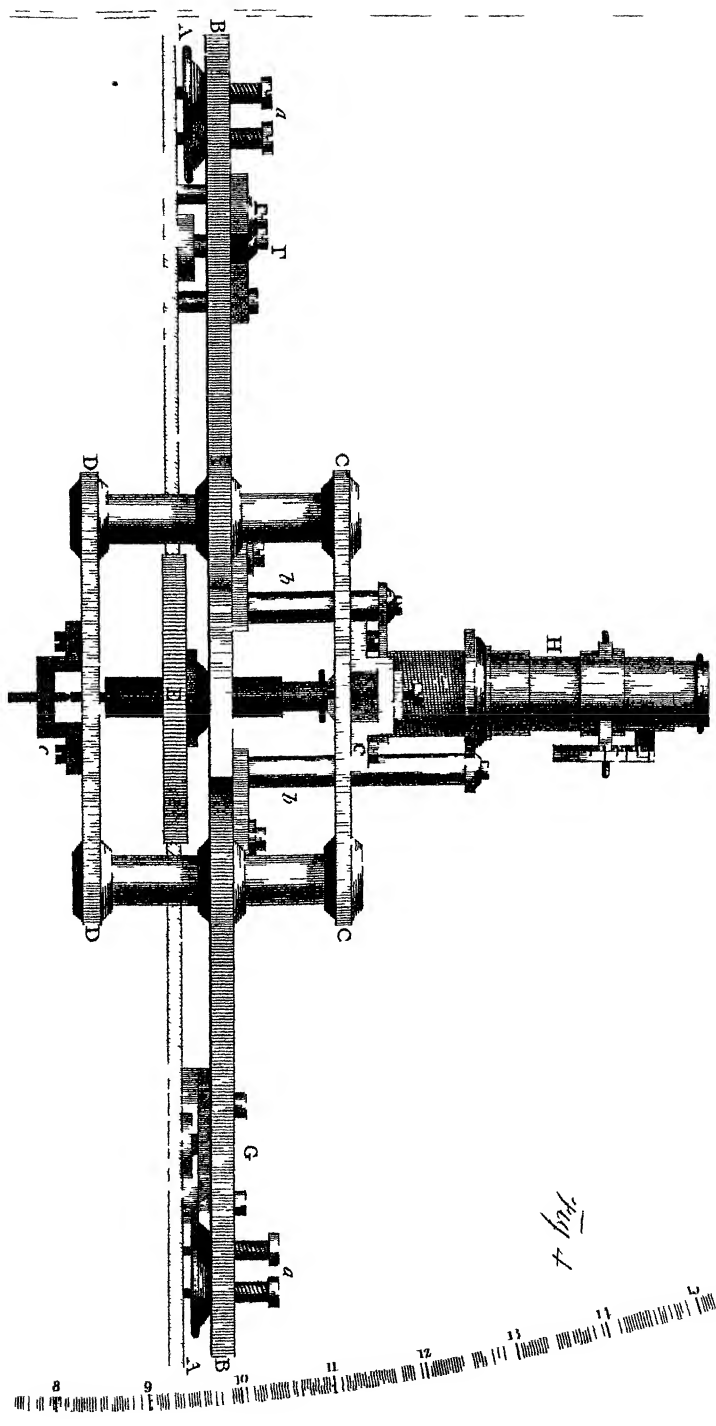| Name of the Dot	First Quadrant.	Second Quadrant.	Third Quadrant.	Fourth Quadrant.	First Quadrant.	Second Quadrant.	Third Quadrant.	Fourth Quadrant.	Name of the Dot
0.0	0,0	+ 8,8	− 6,9	+ 14,4	− 16,9	− 8,0	− 13,4	− 22,4	45,0
1.4	− 4,8	− 0,6	16,0	5,9	8,7	5,5	9,7	16,1	4˙4
2,8	10,2	4,3	24,0	− 2 9	14,3	9,6	17,4	22,3	47.8
4,2	13,8	15,1	28,3	12,8	22,3	17,9	19,9	33.8	49.2
5,6	13,7	12,5	23,3	16,1	26,0	21.6	26,7	31,9	50,6
7,0	15,9	16,8	28,7	19,4	25,5	26,0	23,6	28 9	52 0
8,4	17,6	19,6	32,0	27,0	32,0	27,8	30 3	38,3	53.4
9,8	21,4	16,1	35.5	30,7	34,0	27,3	29.1	35,2	54,8
11,2	21,6	16,7	31,5	26,5	26,8	22,1	24,0	32,6	56 2
12,7	27,9	21,6	32,2	28,6	29,6	24,5	29,7	29,8	57,7
14,1	31,1	26,8	37,5	34,4	33,7	17,7	27,2	24,6	59,1
15,5	28,5	22,7	30,2	26,8	30,2	15,6	29,3	26,5	60,5
16,9	27,3	20,5	32,4	32,7	19,2	15,3	24,1	19,4	61,9
18,3	29,9	18,2	24,2	25,7	21,5	14,6	18,8	23,7	63,3
19,7	20,2	13,5	20,6	22,2	19,0	21,5	22,4	17,4	64,7
21,1	22,4	5,9	22,1	24,0	18,8	19,9	22,8	17,1	66,1
22,5	10,0	1,8	10,9	6,7	3,0	+ 8,2	+ 0,7	+ 2,5	67,5
23,9	8,8	12,2	16,0	14,9	9,8	− 2,8	− 2,5	− 13,0	68,9
25,3	.19,8	15,5	20,2	24,0	15,7	10,2	13,7	19,2	70,3
26,7	21,7	16,1	20,0	33,0	21,9	7,0	21,8	25,8	71,7
28,1	22,1	12,8	23,8	36,4	23,0	13,9	25,1	23,0	73,1
29,5	17,1	15,8	28,9	35,0	27,1	14,3	25,3	26,8	74,5
30,9	22,1	18,0	31,4	37,0	26,6	20,1	26,6	30,7	75,9
32,3	24,7	19,3	33,3	37,7	33,3	21,1	22,7	31,1	77,3
33,7	17,4	9,1	25,1	37,6	27,9	16,0	23 8	29,1	78,7
35,2	22,7	8,0	25,1	35,7	35,5	14,5	18,5	28,7	80,2
36,6	27,3	11,9	27,4	41,8	29,3	9,0	22,4	27,3	81,6
38,0	26,5	15,6	26,9	40,6	21,0	6,6	17,5	21,4	83,0
39,4	26,4	16,7	24,8	43,1	27,5	5,4	21,0	21,6	84,4
40,8	25,4	7,2	25,1	33,6	31,0	7,9	15,4	12,6	85,8
42,2	18,5	10,4	24,7	30,2	23,0	0,1	6,8	5,2	87,1
43,6	16,3	10,0	24,6	31,7	16,3	3,7	15,9	6,4	88,6
45,0	16,9	8,0	13,0	22,4	+ 8,8	6,9	+ 14,4	0,0	90,0

A Letter on a Canal in the Medulla Spinalis of some Quadrupeds. In a Letter from Mr. William Sewell, *to* Everard Home, *Esq. F. R. S.*

Read December 8, 1808.

Sir,

ACCORDING to your request, I send you an account of the icts I have ascertained, respecting a canal I discovered in the ear 1803, in the medulla spinalis of the horse, bullock, sheep, og, and dog; and should it appear to you deserving of being iid before the Royal Society, I shall feel myself particularly ibliged, by having so great an honour conferred upon me.

Upon tracing the sixth ventricle of the brain, which corresponds to the fourth in the human subject, to its apparent termination, the calamus scriptorius, I perceived the appearance of a canal, continuing by a direct course into the centre of the spinal marrow. To ascertain with accuracy whether such structure existed throughout its whole length, I made sections of the spinal marrow at different distances from the brain, and found that each divided portion exhibited an orifice with a diameter sufficient to admit a large sized pin; from which a small quantity of transparent colourless fluid issued, like that contained in the ventricles of the brain. The canal is lined by a membrane resembling the tunica arachnoidea, and is situated above the fissure of the medulla, being separated by a medullary layer: it is most easily distinguished where the large

nerves are given off in the bend of the neck and sacrum, imperceptibly terminating in the cauda equina. Having satisfactorily ascertained its existence through the whole length of the spinal marrow, my next object was to discover whether it was a continued tube from one extremity to the other: this was most decidedly proved, by dividing the spinal marrow through the middle, and pouring mercury into the orifice where the canal was cut across, it passed in a small stream with equal facility towards the brain (into which it entered), or in a contrary direction to where the spinal marrow terminates.

By many similar experiments, I have since proved that a free communication of the limpid fluid, which the canal contains, is kept up between the brain and whole extent of spinal marrow. I have consulted the most celebrated authors on comparative anatomy, but do not find any such structure of those parts described, and as it is not known to you, I may presume that it has not been before taken notice of.

I have the honour to be,

Sir, your obedient faithful servant,

WM. SEWELL.

Veterinary College,
Nov. 26, 1808.

VI. *A numerical Table of elective Attractions; with Remarks on the Sequences of double Decompositions.* *By* Thomas Young, *M. D. For. Sec. R. S.*

Read February 9, 1809.

ATTEMPTS have been made, by several chemists, to obtain a series of numbers, capable of representing the mutual attractive forces of the component parts of different salts; but these attempts have hitherto been confined within narrow limits, and have indeed been so hastily abandoned, that some very important consequences, which necessarily follow from the general principle of a numerical representation, appear to have been entirely overlooked. It is not impossible, that there may be some cases, in which the presence of a fourth substance, besides the two ingredients of the salt, and the medium in which they are dissolved, may influence the precise force of their mutual attraction, either by affecting the solubility of the salt, or by some other unknown means, so that the number, naturally appropriate to the combination, may no longer correspond to its affections; but there is reason to think that such cases are rare; and when they occur, they may easily be noticed as exceptions to the general rules. It appears therefore, that nearly all the phenomena of the mutual actions of a hundred different salts may be correctly represented by a hundred numbers, while, in the usual manner of relating every case as a different experiment, above two thousand separate articles would be required.

Having been engaged in the collection of a few of the principal facts relating to chemistry and pharmacy, I was induced to attempt the investigation of a series of these numbers; and I have succeeded, not without some difficulty, in obtaining such as appear to agree sufficiently well with all the cases of double decompositions which are fully established, the exceptions not exceeding twenty, out of about twelve hundred cases enumerated by FOURCROY. The same numbers agree in general with the order of simple elective attractions, as usually laid down by chemical authors; but it was of so much less importance to accommodate them to these, that I have not been very solicitous to avoid a few inconsistencies in this respect, especially as many of the bases of the calculation remain uncertain, and as the common tables of simple elective attractions are certainly imperfect, if they are considered as indicating the order of the independent attractive forces of the substances concerned. Although it cannot be expected that these numbers should be accurate measures of the forces which they represent, yet they may be supposed to be tolerable approximations to such measures, at least if any two of them are nearly in the true proportion, it is probable that the rest cannot deviate very far from it: thus, if the attractive force of the phosphoric acid for potash is about eight tenths of that of the sulfuric acid for barita, that of the phosphoric acid for barita must be about nine tenths as great; but they are calculated only to agree with a certain number of phenomena, and will probably require many alterations, as well as additions, when all other similar phenomena shall have been accurately investigated.

There is, however, a method of representing the facts, which

have served as the bases of the determination, independently of any hypothesis, and without being liable to the contingent necessity of any future alteration, in order to make room for the introduction of the affections of other substances; and this method enables us also to compare, upon general principles, a multitude of scattered phenomena, and to reject many which have been mentioned as probable, though doubtful, with the omission of a very few only which have been stated as ascertained. This arrangement simply depends on the supposition, that the attractive force, which tends to unite any two substances, may always be represented by a certain constant quantity.

From this principle it may be inferred, in the first place, that there must be a sequence in the simple elective attractions. For example, there must be an error in the common tables of elective attractions, in which magnesia stands above ammonia under the sulfuric acid, and below it under the phosphoric, and the phosphoric acid stands above the sulfuric under magnesia, and below it under ammonia, since such an arrangement implies, that the order of the attractive forces is this; phosphate of magnesia, sulfate of magnesia, sulfate of ammonia, phosphate of ammonia, and again phosphate of magnesia; which forms a circle, and not a sequence. We must therefore either place magnesia above ammonia under the phosphoric acid, or the phosphoric acid below the sulfuric under magnesia; or we must abandon the principle of a numerical representation in this particular case.

In the second place, there must be an agreement between the simple and double elective attractions. Thus, if the fluoric acid stands above the nitric under barita, and below it under

lime, the fluate of barita cannot decompose the nitrate of lime, since the previous attractions of these two salts are respectively greater, than the divellent attractions of the nitrate of barita and the fluate of lime. Probably, therefore, we ought to place the fluoric acid below the nitric under barita; and we may suppose, that when the fluoric acid has appeared to form a precipitate with the nitrate of barita, there has been some fallacy in the experiment.

The third proposition is somewhat less obvious, but perhaps of greater utility: there must be a continued sequence in the order of double elective attractions; that is, between any two acids, we may place the different bases in such an order, that any two salts, resulting from their union, shall always decompose each other, unless each acid be united to the base nearest to it: for example, sulfuric acid, barita, potass, soda, ammonia, strontia, magnesia, glycina, alumina, zirconia, lime, phosphoric acid. The sulfate of potass decomposes the phosphate of barita, because the difference of the attractions of barita for the sulfuric and phosphoric acids is greater than the difference of the similar attractions of potass; and in the same manner the difference of the attractions of potass is greater than that of the attractions of soda; consequently the difference of the attractions of barita must be much greater than that of the attractions of soda, and the sulfate of soda must decompose the phosphate of barita: and in the same manner it may be shown, that each base must preserve its relations of priority or posteriority to every other in the series. It is also obvious that, for similar reasons, the acids may be arranged in a continued sequence between the different bases; and when all the decompositions of a certain number of salts

have been investigated, we may form two corresponding tables, one of the sequences of the bases with the acids, and another of those of the acids with the different bases; and if either or both of the tables are imperfect, their deficiencies may often be supplied, and their errors corrected, by a repeated comparison with each other.

In forming tables of this kind from the cases collected by FOURCROY, I have been obliged to reject some facts, which were evidently contradictory to others, and these I have not thought it necessary to mention; a few, which are positively related, and which are only inconsistent with the principle of numerical representation, I have mentioned in notes; but many others, which have been stated as merely probable, I have omitted without any notice. In the table of simple elective attractions, I have retained the usual order of the different substances; inserting again in parentheses such of them as require to be transposed, in order to avoid inconsequences in the simple attractions: I have attached to each combination marked with an asterisc the number deduced from the double decompositions, as expressive of its attractive force; and where the number is inconsistent with the corrected order of the simple elective attractions, I have also inclosed it in a parenthesis. Such an apparent inconsistency may perhaps in some cases be unavoidable, as it is possible that the different proportions of the masses concerned, in the operations of simple and compound decomposition, may sometimes cause a real difference in the comparative magnitude of the attractive forces. Those numbers, to which no asterisc is affixed, are merely inserted by interpolation, and they can only be so far employed for determining the mutual actions of the salts to which they belong,

as the results which they indicate would follow from the comparison of any other numbers, intermediate to the nearest of those, which are more correctly determined. I have not been able to obtain a sufficient number of facts relating to the metallic salts, to enable me to comprehend many of them in the tables.

It has been usual to distinguish the attractions, which produce the double decompositions of salts, into necessary and superfluous attractions; but the distinction is neither very accurate, nor very important: they might be still further divided, accordingly as two, three, or the whole of the four ingredients concerned are capable of simply decomposing the salt in which they are not contained; and if two, accordingly as they are previously united or separate; such divisions would however merely tend to divert the attention from the natural operation of the joint forces concerned.

It appears to be not improbable, that the attractive force of any two substances might, in many cases, be expressed by the quotient of two numbers appropriate to the substances, or rather by the excess of that quotient above unity; thus the attractive force of many of the acids for the three principal alkalies might probably be correctly represented in this manner; and where the order of attractions is different, perhaps the addition of a second, or of a second and third quotient, derived from a different series of numbers, would afford an accurate determination of the relative force of attraction, which would always be the weaker, as the two substances concerned stood nearer to each other in these orders of numbers; so that, by affixing, to each simple substance, two, three, or at most

four numbers only, its attractive powers might be expressed in the shortest and most general manner.

I have thought it necessary to make some alterations in the orthography generally adopted by chemists, not from a want of deference to their individual authority, but because it appears to me that there are certain rules of etymology, which no modern author has a right to set aside. According to the orthography universally established throughout the language, without any material exceptions, our mode of writing Greek words is always borrowed from the Romans, whose alphabet we have adopted: thus the Greek vowel Υ, when alone, is always expressed in Latin and in English by Y, and the Greek diphthong ΟΥ by U, the Romans having no such diphthong as OU or OY. The French have sometimes deviated from this rule, and if it were excusable for any, it would be for them, since their *u* and *ou* are pronounced exactly as the Υ and ΟΥ of the Greeks probably were: but we have no such excuse. Thus the French have used the term *acoustique*, which some English authors have converted into "acoustics;" our anatomists, however, speak, much more correctly, of the "acustic" nerve. Instead of glucine, we ought certainly, for a similar reason, to write glycine; or glycina, if the names of the earths are to end in *a*. Barytes, as a single Greek word, means weight, and must be pronounced bárytes; but as the name of a stone, accented on the second syllable, it must be written barites; and the pure earth may properly be called barita. Yttria I have altered to itria, because no Latin word begins with a Y.

In all mixtures of the aqueous solutions of two salts, each acid remains united to the base which stands nearest to it in this table.

SULFURIC ACID.

NITRIC
Barita
Strontia
Lime
(Silver?)
(Mercury?)
Potass
Soda
{ Z'nc
  Iron
  Copper? }
Magnesia
Ammonia (1)
Glycina
Alumina
Zirconia

MURIATIC
Barita
Strontia
Lime
Potass
Soda
(Mercury?)
(Iron?)
Magnesia
Glycina
Ammonia (2)
Alumina (2)
Z'rconia
Lime
(Copper?)

PHOSPHORIC
Barita
Potass
Soda
Ammonia
Strontia
Magnesia (3)
Glycina
Alumina
Z'rconia
Lime

FLUORIC
Barita
Potass
Soda
Strontia
Ammonia
Magnesia
Glycina
Alumina
Zirconia
Lime

SULFUROUS
Barita
Potass
Soda
Strontia
Ammonia (4)
Magnesia (4)
Glycina
Alumina
Zirconia
Lime

BORACIC
Barita
Potass
Soda
Strontia
Ammonia (5)
Magnesia
Lime
Glycina
Alumina
Zirconia

CARBONIC
Potass
Soda
Barita
Strontia
Ammonia (6)
Lime
Magnesia?
Glycina
Alumina
Zirconia

(NITROUS)
Barita
Potass
Soda
Strontia
Lime
Magnesia?
Ammonia
Glycina
Alumina
Zirconia

(PHOSPHOROUS)
Barita
Potass
Soda
Strontia
Magnesia
Lime?
Glycina
Ammonia
Alumina
Zirconia

(ACETIC)
Barita
Potass
Soda
Ammonia
Strontia
Magnesia
Glycina
Alumina
Zirconia
Lime?
{ Lead
  Zinc
  Copper }

Lead
Mercury
{ Iron
  Potass }
{ Soda
  Magnesia }

(1) Ammonia stands above magnesia when cold.   (2) A triple salt is formed.   (3) Perhaps magnesia ought to stand lower   (4) A compound salt is formed, and when hot, magnesia stands above ammonia.   (5) FOURCROY says, that sulfate of strontia is decomposed by borate of ammonia.   (6) With heat, ammonia stands below lime and magnesia.

Nitric Acid.

Nitric and Muriatic Acids.

Nitric Acid.					
Barita	Potass	Barita	Potass	Barita (10)	Potass
Potass	Soda	Potass	Soda	Potass	Soda
Soda	Ammonia	Soda	Ammonia	Soda	Barita (10)
Strontia	Magnesia	Ammonia	Magnesia	Ammonia	Ammonia (7,11)
Lime	Glycina	Magnesia	Glycina	Magnesia	Magnesia (7)
Magnesia (7)	Alumina	Glycina	Alumina	Glycina	Strontia
Ammonia (7)	Zirconia (8)	Alumina	Zirconia	Alumina	Lime
Glycina	Barita	Zirconia	Barita	Zirconia	Glycina
Alumina	Strontia	Strontia (9)	Strontia	Strontia	Alumina
Zirconia	Lime	Lime	Lime	Lime	Zirconia
MURIATIC	PHOSPHORIC	FLUORIC	SULFUROUS	BORACIC	CARBONIC

(7) A triple salt is formed. (8) FOURCROY says, that the muriate of zirconia decomposes the phosphates of barita and strontia. (9) According to FOURCROY's account, the fluate of strontia decomposes the muriates of ammonia, and of all the bases below it; but he says in another part of the same volume, that the fluate of strontia is an unknown salt. (10) According to FOURCROY's account of these combinations, barita should stand immediately below ammonia in both of these columns. (11) With heat, the carbonate of lime decomposes the muriate of ammonia.

Phosphoric Acid.

Barita	Lime	Barita	Potass	Barita
Lime	Barita	Lime	Soda	Lime
Potass	Potass	Potass	Barita	Potass
Soda	Soda	Soda	Lime (13)	Soda
Strontia	Strontia	Strontia	Strontia	Strontia
Magnesia	Magnesia	Ammonia (12)	Ammonia	Magnesia
Ammonia	Ammonia	Magnesia	Magnesia	Glycina?
Glycina	Glycina	Glycina	Glycina	Alumina
Alumina	Alumina	Alumina	Alumina	Zirconia
Zirconia	Zirconia	Zirconia	Zirconia	
FLUORIC	SULFUROUS	BORACIC	CARBONIC	(PHOSPHOROUS)

(12) According to FOURCROY, the phosphate of ammonia decomposes the borate of magnesia. (13) FOURCROY says, that the carbonate of lime decomposes the phosphates of potass and of soda.

Fluoric Acid.

Lime	Lime	Potass
Potass	Barita	Soda
Soda	Strontia	Lime
Magnesia	Potass	Barita
Ammonia	Soda	Strontia
Glycina	Ammonia	Ammonia (14)
Alumina	Magnesia	Magnesia
Zirconia	Glycina	Glycina
Strontia	Alumina	Alumina
Barita	Zirconia	Zirconia
SULFUROUS	BORACIC	CARBONIC

(14) According to FOURCROY, the carbonate of ammonia decomposes the fluates of barita and strontia.

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Dr. Young's *Account of a numerical Table*

SULFUROUS ACID.

Barita	Potass	Lime	Zirconia
Strontia	Soda	Strontia	Alumina
Potass	Barita [15]	Barita	Glycina
Soda	Strontia	Zirconia	Ammonia
Ammonia	Ammonia	Alumina	Magnesia
Magnesia	Lime	Glycina	Strontia
Lime	Magnesia	Magnesia	Soda
Glycina	Glycina	Ammonia	Potass
Alumina	Alumina	Soda	Barita
Zirconia	Zirconia	Potass	Lime
BORACIC	CARBONIC	(NITROUS)	(PHOSPHOROUS?)

BORACIC ACID.

Potass
Soda
Lime
Barita
Strontia
Magnesia
Ammonia
Glycina
Alumina
Zirconia
CARBONIC

(15) FOURCROY says, that the sulfite of barita decomposes the carbonate of ammonia.

Table of the Sequences of the Acids with different Bases.

	BARITA			STRONTIA					LIME				POTASS SODA	MAGNESIA	
Sulfuric	S	C	S	S	C	S	P	S	C	P	P	P	POTASS	S	B
Nitric	N	S	P	N	SS	P	S	P	P	F	F	F	SODA	N	C
Muriatic	M	P	SS	M	F	SS	SS	SS	F	B	B	SS	MAGN.=AMM.	M	P
Phosphoric	SS	SS	N	SS	P	F	F	F	B	SS	C	S	GLYCINA	P	F
Sulfurous	P	N	M	C	B	B	B	B	SS	S	SS	B	ALUMINA	F	SS
Fluoric	C	M	F	B	S	C	C	N	S	C	S	N	ZIRCONIA	SS	S
Boracic	B	F	B	F	M	N	N	M	M	N	N	M	Each with every subsequent base in this order	B	N
Carbonic	F	B	C	P	N	M	M	C	N	M	M	C		C	M
STRONTIA	LM	PT	MG	LM	PT	MG	AM	GL	PT	MG	AM	GL			AM
	SD	AM		SD				AL	SD			AL			
		GL						ZR				ZR			
		AL													
		ZR													

The comparative use of this table may be understood from an example: if we suppose that the nitrate of barita decomposes the borate of ammonia, we must place the boracic acid above the nitric, between barita and ammonia in this table, and consequently barita below ammonia, between the fluoric and boracic in the former: hence the boracic and fluoric acids must also be transposed between barita and strontia, and between barita and potass; or if we place the fluoric still higher than the boracic in the first instance, we must place barita below ammonia between the nitric and fluoric acids, where indeed it is not impossible that it ought to stand.

MAGNESIA.		AMMONIA.		GLYCINA?	ALUMINA.	ZIRCONIA?
Oxalic acid	820	Sulfuric acid	808*	Sulfuric acid	718*	709*	700*
Phosphoric		Nitric	731*	Nitric	642*	634*	626*
Sulfuric	810*	Muriatic	729*	Muriatic	639*	632*	625*
(Phosphoric)	736*	Phosphoric	728*	Oxalic	600	594	588
Fluoric		Suberic?	720	Arsenic	580	575	570
Arsenic	733	Fluoric	613*	Suberic?	535	530	525
Mucic	732½	Oxalic	611	Fluoric	534*	529*	524*
Succinic	732¼	Tartaric	609	Tartaric	520	515	510
Nitric	732*	Arsenic	607	Succinic	510	505	500
Muriatic	728*	Succinic	605	Mucic	425	420	415
Suberic?	700	Citric	603	Citric	415	410	405
(Fluoric)	620*	Lactic	601	*Phosphoric*	(648)*	(642)*	(636)*
Tartaric	618	Benzoic	599	Lactic	410	405	400
Citric	615	Sulfurous	433*	Benzoic	400	395	390
Malic?	'600?	Acetic	432	Acetic	395	391	387
Lactic	575	Mucic	431	Boracic	388*	385*	382*
Benzoic	560	Boracic	430*	Sulfurous	355*	351*	347*
Acetic		Nitrous	400	Nitrous	340	336	332
Boracic	459*	Carbonic	339*	Carbonic	325*	323*	321*
Sulfurous	439*	Prussic	270	Prussic	260	258	256
(Acetic)	430						
Nitrous	410						
Carbonic	366*						
Prussic	280						

Numerical Table of elective Attractions.

BARITA.		STRONTIA.		POTASS.	SODA.		LIME.	
Sulfuric acid	1000*	Sulfuric acid	903*	Sulfuric acid			Oxalic acid	960
Oxalic	950	Phosphoric	827'		894*	885*	Sulfuric	868*
Succinic	930	Oxalic	825	Nitric	812*	804*	Tartaric	867
Fluoric		Tartaric	757	Muriatic	804*	797*	Succinic	866
Phosphoric	906*	*Fluoric*		Phosphoric			Phosphoric	865*
Mucic	900	Nitric	754*		801*	795'	Mucic	860
Nitric	849*	Muriatic	748*	Suberic?	745	740	Nitric	741*
Muriatic	840*	(Succinic)	740	Fluoric	671*	666*	Muriatic	736*
Suberic	800	(Fluoric)	703*	Oxalic	650	645	Suberic	735
Citric		*Succinic*		Tartaric	616	611	Fluoric	734*
Tartaric	760	Citric?	618	Arsenic	614	609	Arsenic	733¾
Arsenic	733½	Lactic	603	Succinic	612	607	*Lactic*	732
(Citric)	730	*Sulfurous*	527*	Citric	610	605	Citric	731
Lactic	729	*Acetic*		Lactic	609	604	Malic	700
(Fluoric)	706*	*Arsenic*	(733¼)	Benzoic	608	603	Benzoic	590
Benzoic	597	Boracic	513*	Sulfurous	488*	484*	*Acetic*	
Acetic	594	(Acetic)	480	Acetic	486	482	Boracic	537*
Boracic	(515)*	Nitrous?	430	Mucic	484	480	Sulfurous	516*
Sulfurous	592*	Carbonic	419*	Boracic	482*	479*	(Acetic)	470
Nitrous	450			Nitrous	440	437	Nitrous	425
Carbonic	420*			Carbonic	306*	304*	*Carbonic*	423*
Prussic	400			Prussic	300	298	Prussic	290

SUCCINIC.

Barita	930
Lime	866
Strontia?	740
(Magnesia)	732¼
Potass	612
Soda	607
Ammonia	605
Magnesia	510
Glycina?	505
Alumina	500
Zirconia?	

LACTIC.

Barita	729
Potass	609
Soda	604
Strontia	603
Lime	(732)
Ammonia	601
Magnesia	575
Metallic oxids	
Glycina?	410
Alumina	405
Zirconia	400

SUBERIC.

Barita	800
Potass	745
Soda	740
Lime	735
Ammonia	720
Magnesia	700
Glycina?	535?
Alumina	530
Zirconia?	525?

BENZOIC.

White oxid of arse-nic	
Potass	608
Soda	603
Ammonia	599
Barita	597
Lime	590
Magnesia	560
Glycina?	400?
Alumina	395
Zirconia	390?

CAMPHORIC.

Lime	
Potass	
Soda	
Barita	
Ammonia	
Glycina?	
Alumina	
Zirconia?	
Magnesia	

SULFUROUS.

Barita	592 *
Lime	516 *
Potass	488 *
Soda	484 *
Strontia	(527)*
Magnesia	439 *
Ammonia	433 *
Glycina	355 *
Metallic oxids	351 *
Alumina	347 *
Zirconia	

CITRIC.

Lime	731
Barita	730
Strontia	618
Magnesia	615
Potass	610
Soda	605
Ammonia	603
Glycina?	415?
Alumina	410
Zirconia	405

ACETIC.

Barita	594
Potass	486
Soda	482
Strontia	480
Lime	470
Ammonia	432
Magnesia	430
Glycina	395
Metallic oxids	391
Alumina	387
Zirconia	

Acids.

SULPHURIC.

Barita	1000*
Stronti	903*
Potass	894*
Soda	885*
Lim-	868*
Magnesia	810*
Ammonia	808*
Gly.	718*
Itria	712
Alumin	709*
Zirconi	700*

NITRIC.

Barita	849*
Potass	812*
Soda	804*
Strontia	754*
Lime	741*
Magnesia	732*
Ammonia	731*
Glycina	642*
Alumina	634*
Zirconia	626*

MURIATIC.

Barita	840*
Strontia	804*
Lime	797*
Potass	748*
Soda	736*
Ammonia	729*
Magnesia	728*
Glycina	639*
Alumina	632*
Zirconia	625*

PHOSPHORIC.

Barita	905*
Strontia	827*
Lime	(865)*
Potass	801*
Soda	795*
Ammonia	(728)*
Magnesia	736*
Glycina	648*
Alumina	642*
Zirconia	636*

F

Lime	734*
Barita	706*
Stront	703*
Magn	(620)
Potass	671*
Soda	666*
Ammon	613*
Glycina	534*
Alumina	529*
Zirconia	524*

OXALIC.

Lime	960
Barita	950
Strontia	825
Magnesia	820
Potass	650
Soda	645
Ammonia	611
Glycina?	600
Alumina	594
Zirconia?	588

TARTARIC.

Lime	867
Potass	760
Barita	757
Strontia	618
Magnesia	616
Potass	611
Soda	609
Ammonia	520
Glycina	515
Alumina	510
Zirconia	

ARSENIC.

Barita	733¾
Strontia	733½
Lime	733⅓
Magnesia	733
Potass	614
Soda	609
Ammonia	607
Glycina	580
Alumina	575
Zirconia	570

TUNGSTIC.

Lime	
Barita	
Strontia	
Magnesia	
Potass	
Soda	
Ammonia	
Glycina	
Alumina	
Zirconia	

MUCIC?

Barita	900
Lime	860
Potass	484
Soda	480
Ammonia	431
Glycina	425
Alumina	420
Zirconia	415

BORACIC.

Lime	537 *
Barita	515 *
Strontia	513 *
Magnesia	(459)*
Potass	482 *
Soda	479 *
Ammonia	430 *
Glycina	388 *
Alumina	385 *
Zirconia	382 *

NITROUS?

Barita	450
Potass	440
Soda	437
Strontia	430
Lime	425
Magnesia	410
Ammonia	400
Glycina	340
Alumina	336
Zirconia	332

PHOSPHOROUS.

Lime	300
Barita	298
Strontia	290
Potass	280
Soda	270
Magnesia?	260
Ammonia	258
Glycina	256
Alumina	
Zirconia	

CARBONIC.

Barita	420 *
Strontia	419 *
Lime	(423)*
Potass?	306 *
Soda	304 *
Magnesia	(366)*
Ammonia	339 *
Glycina	325 *
Alumina	323 *
Zirconia	321 *

PRUSSIC.

Barita	400
Strontia	
Potass	
Soda	
Lime	
Magnesia	
Ammonia	
Glycina?	
Alumina	
Zirconia?	

VII. *Account of the Dissection of a Human Fœtus, in which the Circulation of the Blood was carried on without a Heart. By Mr.* B. C. Brodie. *Communicated by* Everard Home, *Esq. F. R. S.*

Read February 16, 1809.

An opportunity lately occurred to me of examining a human fœtus, in which the heart was wanting, and the circulation of the blood was carried on by the action of the vessels only. There have been some other instances of this remarkable deviation from the natural structure; but in that to which I allude the growth of the child had been natural, and it differed much less from the natural formation than in any of those, which are on record, and I have therefore been induced to draw up the following account of it.

A woman was delivered of twins in the beginning of the seventh month of pregnancy. There was a placenta with two umbilical chords, which had their origin about three inches distant from each other. The placenta was not preserved, but Mr. ADAMS, who attended the mother in her lying-in, observed nothing unusual in its appearance. Both fœtuses were born dead. They were nearly of the same size. One of them in no respect differed from the ordinary formation; the other had an unusual appearance, and Mr. ADAMS thought it deserving of examination. Through Dr. HOOPER it was put into my hands for this purpose.

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The fœtus measured thirteen inches from the summit of the cranium to the feet. The thorax and abdomen were surrounded by a large shapeless mass, which concealed the form of the whole upper part of the body. This mass proved to be the integuments covering the posterior part of the neck and thorax, distended with a watery fluid about three pints in quantity, contained in two cysts, lined by a smooth membrane. When the fluid was evacuated, and the cysts allowed to collapse, the fœtus had nearly the natural form. Its extremities had nearly the usual appearance, except that on the right hand there was no thumb; on the left hand there was no thumb also, and only a single finger. There were three toes on the right foot, and four toes on the left foot. The external nostrils consisted only of two folds of skin, under each of which was the orifice of an internal nostril, but pervious only for about half an inch. There was a hare lip, and a cleft in the bony palate extending one third of an inch backwards.

On dissection, the cranium was found somewhat compressed by the fluid contained in the cyst behind it. The brain itself was too putrid for accurate examination, but it was of nearly the natural size, and nothing unusual was observed in it. The membranes had the natural appearance, and the nerves appeared to go off from the brain and spinal marrow nearly as usual.

In the thorax there was no heart, thymus gland, or pleura. The trachea was situated immediately behind the sternum. It had its natural appearance, and divided as usual into the two bronchia. The latter terminated in the lungs, which consisted of two rounded bodies, not more than one third of an inch in diameter, having a smooth external surface, and

composed internally of a dense cellular substance. The œso-phagus had the usual situation, but it terminated in a cul-de-sac at the lower part of the thorax. The rest of the thorax was filled with a dense cellular substance; and in place of the diaphragm, there was a membranous septum between it and the cavity of the abdomen.

In the abdomen, the stomach had no cardiac orifice. The intestine was attached to the mesentery in the usual way; but it was proportionably shorter than natural. There was an imperfect cœcum, but the colon was not distinguished by any difference of structure or appearance from the rest of the in-testine. The rectum had its usual situation in the pelvis. The spleen and renal capsules were small; the kidnies, bladder, penis, and testicles had thé usual appearance. The abdomen was lined by peritonæum, but there was no omentum. The liver and gall-bladder were wanting.

As there was no heart, it became an object of importance to ascertain the exact nature of the circulation: for this pur-pose, the blood-vessels were traced with attention.

The umbilical chord consisted of two vessels only: one of these was larger than the other, and its coats resembled those of a vein, while those of the smaller vessel were thick and elastic, like those of an artery. Both of these vessels entered the navel of the child. The artery passed to the left groin by the side of the urachus, occupying the usual situation of the left umbilical artery. Here it gave off the external and inter-nal iliac arteries of the left side, and was then continued up-wards on the fore-part of the spine forming the aorta. From the aorta arose the common trunk of the right iliac artery, and the branches to the viscera and parietes of the thorax and

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abdomen. At the upper part of the thorax, it sent off the two subclavian, and afterwards divided into the two carotid arteries, without forming an arch. The veins corresponding to these arteries terminated in the vena cava, which was situated on the anterior part of the spine before the aorta, and passed downwards before the right kidney to the right groin. Here it became reflected upwards by the side of the urachus to the navel, and was continued into the larger vessel or vein of the chord.

It appears therefore, that, in this fœtus, not only the heart was wanting, but there was no communication of any kind between the trunks of the venous and arterial systems, as in the natural fœtus, where there is a heart. The only communication between the two sets of vessels, was by means of the capillary branches anastomosing as usual in the fœtus and in the placenta. The blood must have been propelled from the placenta to the child through the artery of the chord, and must have been returned to the placenta by means of the vein, so that the placenta must have been at once the source and the termination of the circulation, and the blood must have been propelled by the action of the vessels only.

It is to be understood, that the circulation in the fœtus receives no propelling power from the action of the heart and arteries of the mother. This, although perfectly known to anatomists, it is proper to mention, as it may not be equally known to all the members of this Society.

It appears extraordinary, that under these circumstances, notwithstanding the circulation through the placenta must have been more languid than is natural, that organ should nevertheless have been capable of exercising its proper func-

tions, so as to produce those changes on the blood, which are necessary for the maintenance of fœtal life. This may be explained by considering that in the natural fœtus the umbilical arteries are branches of the general arterial system, and only a portion of the blood of the child is sent to the placenta, whereas in the fœtus which I have described, the trunk of the vena cava was continued into the vein of the chord, and the whole of the venous blood circulated through the placenta, and was exposed to the influence of the arterial blood of the mother.

But the most interesting circumstance, which we learn from this examination is, that the circulation not only can be carried on without a heart, but that a child so circumstanced can be maintained in its growth, so as to attain the same size as a fœtus which is possessed of that organ. This fact is contrary to what prior experience has led us to expect, as will appear from the following abstract of the authenticated cases of this species of malformation, which we find on record.

A monster, in which there was no heart, is described by M. MERY.* There were twins, one of which was well formed, and of the usual size of a six month's child: the size of the other was not mentioned, so that no comparison could be made between them. In the latter, the head, neck, and upper extremities were wanting. There were no vestiges of a brain, nor was there any liver. The dissection of the blood-vessels does not appear to have been very accurately made, but from the general account I should suppose, that the circulation did not materially differ from that of the fœtus which I have described.

* *Histoire de l'Academie Royale des Sciences,* 1720.

Another instance of this kind is described by M. Winslow.*
This was also a twin, only seven inches in length. The age
and size of the other child are not mentioned. In this instance
there was no head, nor any vestige of brain. There were no
lungs, liver, stomach, nor spleen, and only a small portion of
intestine. The arterial system is described as being complete,
communicating with the placenta by the umbilical vein open-
ing into the aorta, and the umbilical arteries arising nearly as
usual. In this instance there was a circle of vessels formed
by the arteries only, for M. Winslow expressly states, that
there were no veins; and however extraordinary this may ap-
pear, we cannot be otherwise than cautious in denying an
observation made by an anatomist, so remarkable for his
extreme accuracy and minuteness.

Dr. Le Cat of Rouen, states another case of twins † born
at the end of the ninth month of pregnancy. One of them
was a well formed child of the usual size; but the other was
only twelve inches and a half in length. The head of the
latter was very imperfect, and there was only a very minute
portion of brain. The heart, lungs, liver, stomach, and spleen
were entirely wanting, and there was only a small portion of
intestine. The arterial system was perfect; the umbilical
vein terminated in the aorta, and the umbilical arteries had
their origin from the internal iliac, as usual. There is, how-
ever, an obscurity in the account of the circulation, as it is
stated that there were veins, but they were not traced, nor
was any communication made out between them and the ar-
teries or the vessels of the chord.

Dr. CLARKE* has given an account of a case, in which a woman, after a natural labour, was delivered of a healthy child, and also of a substance covered by common integuments, of an oval form, four inches in length, and having a separate navel string and placenta. In this substance there was one os innominatum, with a femur, tibia, and fibula. There were neither brain nor nerves; nor were there any viscera, except a small portion of intestine. The umbilical chord consisted of two vessels, an artery and a vein, both of which ramified in this substance and in the placenta.

In Dr. HUNTER's anatomical collection, there are two specimens of monsters born without hearts. In both of them the whole upper part of the body was wanting; and in neither was the exact nature of the circulation ascertained.

In each of the instances which I have quoted, not only the heart was wanting, but the fœtus in other respects was so imperfect, that it could not be considered as any thing more than a mola, or an irregularly formed living mass connected with the placenta. In particular, in all of them the brain, which may with justice be considered as affording the best distinction between a mola and a fœtus, was wanting; whereas in that which forms the subject of the present paper, the brain was nearly as large as usual, and in other respects the fœtus varied much less from the natural structure, than in any former instance.

In the cases already on record, we have seen, that wherever the size of the monster was mentioned, it was much smaller than a natural fœtus. This would have led to the supposition, that a circulation, which was carried on by the action of the vessels only, was incapable of maintaining the

* Phil. Trans. for 1793.

natural growth of a child, had it not been found that the fœtus, which I have described, though the heart was wanting, was fully equal in size to a fœtus of the same age, which was possessed of that organ.

It may be observed, that in all these cases, in which the heart was wanting, the liver was wanting also. It is probable, that the action of the vessels only, without the assistance of the heart, would have been insufficient to propel the blood through the circulation of the liver, which is so extensive in the natural fœtus.

VIII. *On the Origin and Formation of Roots. In a Letter from* T. A. Knight, *Esq. F. R. S. to the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.*

Read February 23, 1809.

MY DEAR SIR,

IN a former communication I have given an account of some experiments, which induced me to conclude that the buds of trees invariably spring from their alburnum, to which they are always connected by central vessels of greater or less length; and in the course of much subsequent experience, I have not found any reason to change the opinion that I have there given.* The object of the present communication is to shew, that the roots of trees are always generated by the vessels which pass from the cotyledons of the seed, and from the leaves, through the leaf-stalks and the bark, and that they never, under any circumstances, spring immediately from the alburnum.

The organ, which naturalists have called the radicle in the seed, is generally supposed to be analogous to the root of the plant, and to become a perfect root during germination; and I do not know that this opinion has ever been controverted, though I believe that, when closely investigated, it will prove to be founded in error.

A root, in all cases with which I am acquainted, elongates only by new parts which are successively added to its apex or

* Phil. Trans. 1805.

point, and never, like the stem or branch, by the extension of parts previously organized; and I have endeavoured to shew, in a former memoir, that owing to this difference in the mode of the growth of the root and lengthened plumule of germinating seeds, the one must ever be obedient to gravitation, and point towards the centre of the earth, whilst the other must take the opposite direction.* But the radicle of germinating seeds elongates by the extension of parts previously organised, and in a great number of cases, which must be familiar to every person's observation, raises the cotyledons out of the mould in which the seed is placed to vegetate. The mode of growth of the radicle is therefore similar to that of the substance which occupies the spaces between the buds near the point of the succulent annual shoot, and totally different from that of the proper root of the plant, which I conceive to come first into existence during the germination of the seed, and to spring from the point of what is called the radicle. At this period, neither the radicle nor cotyledons contain any alburnum ; and therefore the first root cannot originate from that substance; but the cortical vessels are then filled with sap, and apparently in full action, and through these the sap appears to descend which gives existence to the true root.

When first emitted, the root consists only of a cellular substance, similar to that of the bark of other parts of the future tree, and within this the cortical vessels are subsequently generated in a circle, inclosing within it a small portion of the cellular substance, which forms the pith or medulla of the root. The cortical vessels soon enter on their office of gene-

* Phil. Trans. 1806.

rating alburnous matter; and a transverse section of the root then shews the alburnum arranged in the form of wedges round the medulla, as it is subsequently deposited on the central vessels of the succulent annual shoot, and on the surface of the alburnum of the stems and branches of older trees.*

If a leaf-stalk be deeply wounded, a cellular substance, similar to that of the bark and young root is protruded from the upper lip of the wound, but never from the lower; and the leaf-stalks of many plants possess the power of emitting roots, which power can not have resided in alburnum, for the leaf-stalk does not contain any; but vessels, similar to those of the bark and radicle, abound in it, and apparently convey the returning sap; and from these vessels, or perhaps more properly from the fluid they convey, the roots emitted by the leaf-stalk derive their existence.†

If a portion of the bark of a vine, or other tree, which readily emits roots, be taken off in a circle extending round its stem, so as to intercept entirely the passage of any fluid through the bark; and any body which contains much moisture be applied, numerous roots will soon be emitted into it immediately above the decorticated space, but never immediately beneath it: and when the alburnum in the decorticated spaces has become lifeless to a considerable depth, buds are usually protruded beneath, but never immediately above it, apparently owing to the obstruction of the ascending sap. The roots, which are emitted in the preceding case, do not appear in any degree to differ from those which descend from the radicles of generating seeds, and both apparently derive

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* Phil. Trans. for 1801, Plate 27.

† Phil. Trans. for 1801.

their matter from the fluid which descends through the corti-
cal vessels.

There are several varieties of the apple tree, the trunks and
branches of which are almost covered with rough excres-
cences, formed by congeries of points which would have be-
come roots under favourable circumstances; and such varieties
are always very readily propagated by cuttings. Having
thus obtained a considerable number of plants of one of these
varieties, the excrescences began to form upon their stems
when two years old, and mould being then applied to them
in the spring, numerous roots were emitted into it early in the
summer. The mould was at the same time raised round,
and applied to, the stems of other trees of the same age and
variety, and in every respect similar, except that the tops
of the latter were cut off a short distance above the lowest
excrescence, so that there were no buds or leaves from which
sap could descend to generate or feed new roots; and under
these circumstances no roots, but numerous buds were emitted,
and these buds all sprang from the spaces and points, which
under different circumstances had afforded roots. The tops
of the trees last mentioned, having been divided into pieces
of ten inches long, were planted as cuttings, and roots were
by these emitted from the lowest excrescences beneath the
soil, and buds from the uppermost of those above it.

I had anticipated the result of each of the preceding expe-
riments; not that I supposed, or now suppose, that roots can
be changed into buds, or buds into roots; but I had before
proved that the organization of the alburnum is better calcu-
lated to carry the sap it contains, from the root upwards, than
in any other direction, and I concluded that the sap when

arrived at the top of the cutting through the alburnum would be there employed, as I had observed in many similar cases, in generating buds, and that these buds would be protruded where the bark was young and thin, and consequently afforded little resistance.* I had also proved the bark to be better calculated to carry the sap towards the roots than in the opposite direction, and I thence inferred that as soon as any buds, emitted by the cuttings, afforded leaves, the sap would be conveyed from these to the lower extremity of the cuttings by the cortical vessels, and be there employed in the formation of roots.†

Both the alburnum and bark of trees evidently contain their true sap; but whether the fluid which ascends in such cases as the preceding through the alburnum to generate buds, be essentially different from that which descends down the bark to generate roots, it is perhaps impossible to decide. As nature, however, appears in the vegetable world to operate by the simplest means; and as the vegetable sap, like the animal blood, is probably filled with particles which are endued with life, were I to offer a conjecture, I am much more disposed to believe that the same fluid, even by merely acquiring different motions, may generate different organs, than that two distinct fluids are employed to form the root, and the bud and leaf.

When alburnum is formed in the root, that organ possesses, in common with the stem and branches, the power of producing buds, and of emitting fibrous roots, and when it is detached from the tree, the buds always spring near its upper end, and the roots near the opposite extremity, as in the

* Phil. Trans. for 1805.

cuttings abovementioned. The alburnum of the root is also similar to that of other parts of the tree, except that it is more porous, probably owing to the presence of abundant moisture during the period in which it is deposited.* And possibly the same cause may retain the wood of the root permanently in the state of alburnum; for I have shewn, in a former memoir, that if the mould be taken away, so that the parts of the larger roots, which adjoin the trunk, be exposed to the air, such parts are subsequently found to contain much heart wood.*

I would wish the preceding observations to be considered as extending to trees only, and exclusive of the palm tribe; but I believe they are nevertheless generally applicable to perennial herbaceous plants, and that the buds and fibrous roots of these originate from substances which correspond with the alburnum and bark of trees. It is obvious, that the roots which bulbs emit in the spring, are generated by the sap which descends from the bulb, when that retains its natural position; and such tuberous rooted plants as the potatoe offer rather a seeming than a real obstacle to the hypothesis I am endeavouring to establish. The buds of these are generally formed beneath the soil; but I have shewn, in a former memoir, that the buds on every part of the stem may be made to generate tubers, which are similar to those usually formed beneath the soil; and I have subsequently seen, in many instances, such emitted by a re-produced bud, without the calix of a blossom, which had failed to produce fruit; but I have never, under any circumstances, been able to obtain tubers from the fibrous roots of the plant.

The tube therefore appears to differ little from a branch,

* Phil. Trans. for 1801.

which has dilated instead of extending itself, except that it becomes capable of retaining life during a longer period; and when I have laboured through a whole summer to counteract the natural habits of the plant; a profusion of blossoms has in many instances sprung from the buds of a tuber.

The runners also, which, according to the natural habit of the plant, give existence to the tubers beneath the soil, are very similar in organization to the stem of the plant, and readily emit leaves and become converted into perfect stems, in a few days, if the current of ascending sap be diverted into them; and the mode in which the tuber is formed above, and beneath the soil, is precisely the same. And when the sap, which has been deposited at rest during the autumn and winter, is again called into action to feed the buds, which elongate into parts of the stems of the future plants in the spring, fibrous roots are emitted from the bases of these stems, whilst buds are generated at the opposite extremities, as in the cases I have mentioned respecting trees.

Many naturalists* have supposed the fibrous roots of all plants to be of annual duration only; and those of bulbous and tuberous rooted plants certainly are so: as in these nature has provided a distinct reservoir for the sap which is to form the first leaves and fibrous roots of the succeeding season; but the organization of trees is very different, and the alburnum and bark of the roots and stems of these are the reservoirs of their sap during the winter.† When, however, the fibrous roots of trees are crowded together in a garden-pot, they are often found lifeless in the succeeding spring; but I

* M. MIRBEL's *Traité d'Anatomie,* &c. &c. Dr. SMITH's Introduction to Botany.
† Phil. Trans. for 1805.

have not observed the same mortality to occur, in any degree, in the roots of trees when growing, under favourable circumstances, in their natural situation.

I am prepared to offer some observations on the causes which direct the roots of plants in search of proper nutriment and which occasion the root of the same plant to assume different forms under different circumstances; but I propose to make those observations the subject of a future communication.

I am, MY DEAR SIR,

with great respect,

your much obliged, &c. &c.

THOMAS AND. KNIGHT.

Elton, Dec. 22, 1808.

IX. *On the Nature of the intervertebral Substance in Fish and Quadrupeds.* *By* Everard Home, *Esq. F. R. S.*

Read February 23, 1809.

In examining the internal structure of a Squalus maximus of Linnæus, that lately came under my observation, a description of which will be the subject of a future paper, I met with a peculiarity in the intervertebral substance of the spine not hitherto made known to the public.

The fish is thirty feet six inches long, the diameter of the larger vertebræ near the head, seven inches. The intervertebral substance was cut into by Mr. CLIFT four days after the fish was brought on shore, and a limpid fluid rushed out with so much velocity, that it rose to the height of four feet.

At the end of twelve days, I had an opportunity of examining a portion of the spine, the intervertebral joints of which were preserved entire; upon sawing through two of the vertebræ, a fluid was met with, of the consistence of liquid jelly with clots of different sizes floating in it, so that in eight days a considerable tendency to coagulation had taken place, although the fluid was entirely excluded from the external air.

The form of the cavity thus exposed by a longitudinal section being made of it, is nearly spherical, capable of containing three pints of liquid, the lateral parts are ligamentous and elastic, uniting together the edges of the concave surfaces of

the two contiguous vertebræ. When the liquid is evacuated, the elasticity of the lateral ligaments brings the ends of the vertebræ within an inch and half of each other; in this state the inner layers of the ligaments, which are less firm in texture than the outer, project into the cavity, and may be mistaken for a part of its natural contents; this portion when soaked in water swells out to a considerable size, the water readily insinuating itself between the membranous layers of which it is composed.

The whole thickness of the lateral ligaments is about one inch, the external half of which is compact and elastic, the other appears to possess a very slight degree of elasticity. The appearance of the joint is shewn in the annexed drawing, and an account of the analysis of the fluid by Mr. W. BRANDE forms a postscript to this paper. Every part of the mechanism is formed upon so large a scale, that it is rendered conspicuous, and nothing is left to doubt or conjecture; the nature of the joint is different from every other that is met with in animal bodies, and there are many circumstances respecting it, which render it uncertain whether human ingenuity can ever make any resemblance to it, that can be applied to the purposes of mechanics.

These would have been sufficient grounds for bringing this subject before the Society; but there are others of still greater importance which have induced me to make it a separate communication; it enables us to explain the general principle upon which all intervertebral joints are formed, which has been hitherto but imperfectly understood. With this view, I will first describe the principle upon which this particular joint is formed, and then shew the resemblance that it bears to

those of other animals, in which the parts are not so readily distinguished from one another, and consequently their precise use has not been accurately ascertained.

The fluid contained in the cavity being incompressible, preserves a proper interval between the vertebræ to allow of the play of the lateral elastic ligaments, and forms a ball round which the concave surfaces of the vertebræ are moved, and readily adapts itself to every change which takes place in the form of the cavity.

The elasticity of the ligaments, by its constant action, renders the joint always firm, independent of any other support, and keeps the ends of the vertebræ opposed to each other, so that the whole spine is preserved in a straight line, unless it is acted on by muscles or some other power. When a muscular force is applied to one side of the spine, it stretches the elastic ligament on the opposite side of the joint, and as soon as that force ceases to act, the joint returns to the former state. This is one of the most beautiful instances in nature of elasticity being employed as a substitute for muscular action.

The extent of the motion in each particular joint is undoubtedly small, but this is compensated by their number, and the elasticity of the vertebræ themselves.

Fish in general have their vertebræ formed with similar concavities to those of the squalus maximus; these, when examined after death, contain a solid jelly, but in the living fish it is found in a fluid state. This fact was ascertained in the skate, the smallness of the quantity of fluid in any one joint, and the readiness with which it coagulates after death, prevented it from being before observed: the fluid in the

skate is found by Mr. W. Brande to have the same properties, as far as the small quantity that can be collected admits of examination, with that in the squalus maximus.

Although this structure of the intervertebral joint appears to be common to fish in general; the form of the cavity is not in all exactly the same; in the skate it is very similar to that in the squali, but in the common eel, it is more oblong, the longitudinal diameter being about one third longer than the transverse one.

It is evidently contrived for producing the quick vibratory lateral motion, which is peculiar to the back bones of fish while swimming, and enables them to continue that motion for a great length of time, with a small degree of muscular action.

In the sturgeon, there are some curious peculiarities in the structure of the spine. Externally there is the common appearance of regular vertebræ, but these prove to be only cartilaginous rings, the edges of which are nearly in contact, and are united together by elastic ligaments, forming a tube the whole length of the spine, this is lined throughout its internal surface with a firm compact elastic substance, about the thickness of the cartilaginous tube, within this is a soft flexible substance in a small degree elastic; in the centre there is a chain of cavities in the form of lozenges, containing a fluid, and communicating with one another by very small apertures bearing a slight similarity to the intervertebral cavities of the spine in other fish.

As all the different parts of which this spine is composed are more or less elastic, except the central fluid, it must have great flexibility adapting it to the motions of this particular

fish. The structure of the spine in the lamprey eel resembles that of the sturgeon.

The intervertebral joint which is common to fish, is not met with in any of the whale tribe, whose motion through the water is principally effected by means of their horizontal tail; in them the substance employed to unite the vertebræ together is the same as in quadrupeds in general, and from the size of the vertebræ it is on a larger scale, and rendered more conspicuous.

The external portion is very firm and compact, is ranged in concentric circles with transverse fibres uniting the layers together, it becomes softer towards the middle, and in the centre there is a pliant soft substance without elasticity, but admitting of extension more like a jelly than an organized body, corresponding in its use to the incompressible fluid in the fish.

To ascertain whether this structure was generally met with in the spines of quadrupeds, Mr. BRODIE, at my request, examined the intervertebral substance in a great many animals, and found, what, undoubtedly, was very little to be expected, that in the hog and rabbit, in the central part, there is a cavity with a smooth internal surface of the extent of half the diameter of the vertebra, in which is contained a thick gelatinous fluid, so that in some quadrupeds there is an approach towards the intervertebral joint in fish; but whether this is to answer any essential purpose to these animals, or is only to form an intermediate link in the chain of gradation of structures, which is so uniformly adhered to in the productions of nature, cannot at present be determined.

In the bullock, sheep, deer, monkey, and man, the struc-

ture corresponds with that of the whale; in the three last, the central substance appears to be the most compact. Besides the structures already mentioned, there is in some animals one of a very different kind; in the alligator the vertebræ through the whole length of the spine, have regular joints between them, the surfaces are covered with articulating cartilages; and there is synovia and a capsular ligament. In the snake, there is a regular ball and socket joint between every two vertebræ; so that the means employed for the motion of the back bone in different animals, comprehends almost every species of joint with which we are acquainted.

Having mentioned a sufficient number of facts to point out the animals, in which the different structures of the intervertebral substance are to be found, I have abstained from being more particular in my account; as it would in no respect elucidate the principal object of the present communication.

From the facts and observations which have been stated, it appears that the intervertebral substance of the human spine does not consist entirely of elastic ligament, dense in its texture at the circumference, and becoming gradually softer towards the centre; but that the middle portion is composed of a substance which renders it very pliant, though not at all elastic, fixing it to the vertebræ at the proper distance from each other, so as to admit of the action of the lateral elastic ligaments.

When this knowledge is applied to the treatment of curvatures of the spine, a complaint so commonly met with in persons whose strength does not bear the necessary proportion to the growth of the body, it will shew the great advantage of strengthening the intervertebral ligaments,

since in that state the central substance no longer supports the vertebræ, and the joints must lose their proper firmness, which will be attended with many disadvantages.

As the principal motive which induces me to prosecute the laborious researches of comparative anatomy, is to attain a more complete knowledge of the structure and functions of the human body, than can be acquired in any other way, and to apply that knowledge to the most useful of all purposes, the cure of diseases, the success which has attended my labours, in the present instance, affords me particular satisfaction; it encourages me in the pursuit of those inquiries, and holds out an invitation to others, by showing them that the paths of nature, however frequently they have been traced, are not yet sufficiently explored.

EXPLANATION OF THE PLATE.

A longitudinal section of one of the intervertebral joints of the squalus maximus, after the fluid had been evacuated, and the parts had been steeped in water.

a a a a. The section of the vertebra to show its shape and the two concave surfaces which form the intervertebral cavities. The vertebra itself is partly bone, and partly transparent cartilage; the bony portion forms the two cup-like cavities, and the intermediate substance consists of bony cells in form of lozenges filled with cartilage.

The cavity of the joint is in its contracted state, and the inner portion of the lateral ligaments, which is made up of thin layers of a loose texture, has its interstices loaded with water,

which makes it project into the cavity of the joint more than it could do in a natural state.

The external portion of the ligament, to the thickness of half an inch, is the only truly elastic part on which its strength depends.

A chemical Analysis of the Fluid contained in the intervertebral Cavity of the Squalus maximus. By Mr. William Brande.

The fluid found in the intervertebral cavities is of an opal colour; it is semi-transparent, and has a strong fishy smell and taste.

Its specific gravity is ,1027.

In the first instance it does not readily mix with water; but is easily diffused through that fluid by agitation.

When heated in a water bath to a temperature of 212°, it becomes more transparent, but undergoes no farther apparent change.

Infusion of galls and of catechu produce no alteration in it.

Solution of oxymuriat of mercury occasioned a very copious white precipitate, and a similar effect was produced by a solution of oxymuriat of tin.

Nitrat of silver and acetat of lead threw down precipitates of muriat of silver and of lead.

Muriatic acid occasioned a slight cloud after two hours had elapsed, and after twenty-four hours, a small quantity of white matter separated.

Boiled produced no change,

with a solution of pure potash, a
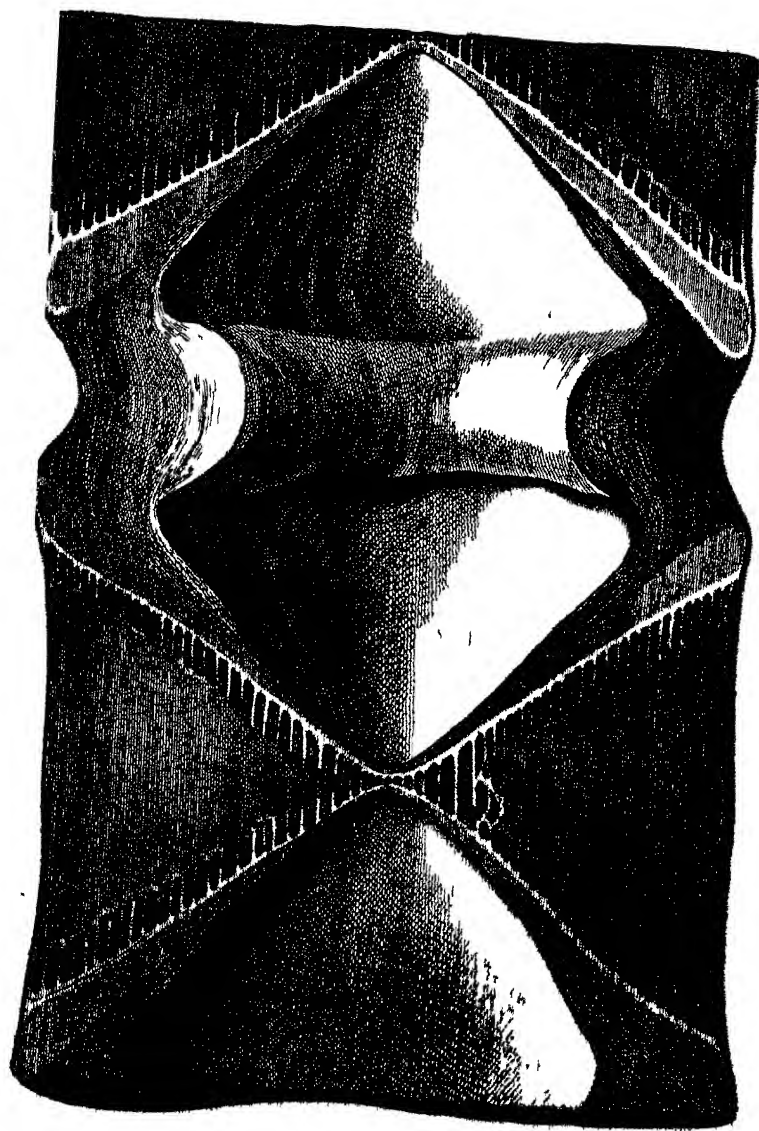
Scale two thirds of an Inch to one Inch

small quantity of ammonia being at the same time evolved. Muriatic acid did not produce any immediate precipitation in this alkaline solution.

The effect of these re-agents, evidently proves the non-existence of gelatine in this fluid; it would also appear that it contains no albumen, unless the effects produced by muriatic acid and by the oxymuriat of mercury and of tin, be regarded as indications of that substance.

It seems to approach nearer to *mucus* or *mucilage*, than to any other animal fluid.*

When the fluid is evaporated in a temperature not exceeding 220° to half its bulk, an opaque substance in the form of bluish white filaments, gradually separates. A thin semi-transparent pellicle forms at the same time upon the surface, which, when removed, is soon succeeded by another. These pellicles were dried on bibulous paper.

The fluid part, remaining after the separation of the filamentous substance and pellicles, afforded a very distinct yellowish cloud, with solutions containing tannin. It was somewhat turbid, but did not form any deposit. In other respects, it nearly resembled the original fluid before evaporation.

The filaments which appeared during evaporation, were separated by passing the fluid through a piece of fine muslin. They resembled albumen imperfectly coagulated, not only in appearance, but in most of their chemical properties.

When the fluid began to putrify, a considerable quantity of the same substance separated spontaneously.

* By *mucus of animals,* I mean a glary fluid, which does not mix readily with water, which is neither coagulated by heat or acids, and which does not form a precipitate with solutions containing tannin.

This substance was insoluble in water, and when boiled for a few minutes in that fluid, it became whiter, harder, and more opaque.

It underwent the same change in alcohol, and when boiled in alcohol, or in dilute muriatic acid, it became still more firm, and appeared like perfectly coagulated albumen.

In this state it was soluble in a solution of pure potash, forming a saponaceous compound, which was decomposed by dilute muriatic acid, a white flaky precipitate being formed. It possessed the other properties which Mr. HATCHETT has enumerated as belonging to coagulated albumen.*

When the pellicle, which had formed on the surface of the fluid during evaporation, was nearly dry, it became somewhat tough and elastic; it was semi-transparent, and of a dirty white colour.

When boiled for some time in water, about three fourths of it were found to be soluble in that fluid, the remainder, when separated by filtration, possessed the properties of the albuminous substance already mentioned.

The solution afforded a copious precipitate with solutions containing tannin. It was not at first affected, either by oxy-muriat of mercury, or of tin; but after twenty four hours, a slight deposit took place.

Although these re-agents indicated the presence of a substance having the properties of pure gelatine in solution, yet it could not be brought to gelatinize by the usual method of evaporation.

From these experiments it would appear, that the inter-vertebral fluid, is of a peculiar nature; that in its original

* Vide Phil. Trans. 1800

properties it resembles mucus, but that under certain circumstances it is capable of being converted into modifications of gelatine and albumen.

The intervertebral fluid of the skate was found to resemble mucus; it did not exhibit any traces of albumen, but the quantity which I procured for examination being very small, I was unable to ascertain its further analogies to the fluid found in the intervertebral cavities of the Squalus maximus.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

a

METEOROLOGICAL JOURNAL

for January, 1808.

1808	Six's Therm. least and greatest Heat.	Time. H.	Time. M	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Jan. 1	43	8	0	43	50	29,20	66	0,095	S	2	Rain.
	47	2	0	47	53	29,23	63		S	2	Fair.
2	45	8	0	45	51	28 90	65	0,155	S	2	Rain.
	45	2	0	44	53	29,03	60		WSW	2	Fair.
3	34	8	0	34	51	29,25	66	0,233	WSW	1	Fine.
	42	2	0	42	53	29,35	60		WSW	1	Cloudy.
4	36	8	0	34	51	29,93	63		W	1	Cloudy.
	39	2	0	39	53	29,90	61		S	1	Fine.
5	39	8	0	46	51	29,80	73		SSW	1	Cloudy.
	50	2	0	49	53	29,68	74		S	1	Rain.
6	41	8	0	41	52	30,25	64		SSW	1	Cloudy.
	48	2	0	43	53	30,34	62		WNW	1	Cloudy.
7	42	8	0	44	53	30,46	70		SSW	1	Cloudy.
	49	2	0	49	55	30,47	72		SSW	1	Cloudy.
8	44	8	0	45	54	30,51	74		W	1	Foggy.
	46	2	0	46	56	30,51	71		W	1	Cloudy.
9	40	8	0	41	53	30,50	74		W	1	Foggy.
	47	2	0	47	55	30,50	71		NW	1	Cloudy.
10	44	8	0	44	53	30,43	73		W	1	Cloudy.
	49	2	0	47	56	30,27	67		SW	1	Cloudy.
11	46	8	0	48	54	29,95	70		WNW	1	Cloudy.
	47	2	0	47	56	29,98	58		NW	1	Cloudy.
12	37	8	0	37	53	29,92	63		N	1	Fair.
	39	2	0	39	55	29,90	58		NW	1	Fair.
13	35	8	0	37	53	29,78	65		W	1	Cloudy.
	46	2	0	44	56	29,80	63		WNW	1	Fair.
14	41	8	0	43	53	29,20	65	0,072	WSW	1	Cloudy.
	47	2	0	47	56	29,12	64		W	1	Rain.
15	30	8	0	30	50	29 96	55	0,020	N	2	Fine.
	34	2	0	34	52	30,07	53		N	2	Fine.
16	26	8	0	26	48	30,16	62		N	1	Cloudy.
	33	2	0	32	50	30,12	60		NW	1	Cloudy.

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1808	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Jan. 17	26	8	0	26	48	30,35	63		NE	1	Fine.
	36	2	0	36	51	30,43	58		NE	1	Fair.
18	27	8	0	27	47	30,48	65		NE	1	Fair.
	35	2	0	35	49	30,44	63		N	1	Fair.
19	29	8	0	35	47	30,22	66		WNW	1	Foggy.
	41	2	0	40	50	30,10	67		WSW	1	Cloudy.
20	38	8	0	38	48	29,65	70	0,063	W	1	Cloudy.
	41	2	0	41	51	29,60	62		NW	1	Fair.
21	23	8	0	23	47	29 82	58		NNE	1	Cloudy.
	30	2	0	37	49	29,91	60		NW	1	Cloudy.
22	18	8	0	18	46	30,17	60		W	1	Cloudy.
	33	2	0	27	48	30,17	55		W	1	Fair.
23	23	8	0	31	46	30,04	63		SW	1	Fair.
	40	2	0	38	49	29,94	67		SSW	1	Cloudy.
24	36	8	0	36	47	29,83	73	0,016	SW	1	Foggy.
	40	2	0	40	50	29,75	73		SW	1	Cloudy.
25	34	8	0	34	47	29,42	73	0,280	SW	1	Cloudy.
	40	2	0	38	49	29,34	72		SW	1	Cloudy.
26	28	8	0	28	47	29,20	68		SE	1	Cloudy.
	34	2	0	33	49	29,28	63		W	1	Fair.
27	26	8	0	27	46	29,51	65		WNW	1	Fair.
	42	2	0	37	51	29,52	62		SW	1	Fair.
28	37	8	0	47	47	29,22	70		SW	2	Cloudy.
	48	2	0	46	51	29,25	58		W	2	Cloudy.
29	36	8	0	36	49	29,51	61		W	2	Fine.
	43	2	0	43	52	29,64	55		WNW	2	Fine.
30	40	8	0	41	50	29,73	67	0,080	SW	1	Rain.
	51	2	0	50	53	29,64	72		SW	1	Cloudy.
31	49	8	0	49	51	29,69	73	0,043	SW	2	Cloudy.
	53	2	0	52	56	29,77	64		W	2	Cloudy.

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for February, 1808.

1808	Sun's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. o	Therm. within. o	Barom. Inches.	Hygrometer. o	Rain. Inches.	Winds. Points	Winds. Str.	Weather.
Feb. 1	47	7	0	48	52	29 86	73		SSW	1	Cloudy.
	53	2	0	52	56	29,87	67		SW	1	Cloudy.
2	48	7	0	48	55	29,64	65		SSW	2	Cloudy.
	53	2	0	50	57	29,57	60		SSW	2	Rain.
3	40	7	0	40	53	29 77	62	0,065	W	2	Fair.
	47	2	0	47	57	29,58	55		W	2	Fair.
4	33	7	0	35	53	30,24	65		W	3	Fair.
	41	2	0	41	54	30 34	64		SW	1	Fine.
5	37	7	0	43	53	30,17	65		SW	1	Cloudy.
	51	2	0	48	55	30 13	65		SW	1	Cloudy.
6	37	7	0	41	53	29 96	63		SW	2	Cloudy.
	51	2	0	51	55	29 78	64		SW	2	Cloudy.
7	48	7	0	48	54	29 78	67	0,020	SW	2	Cloudy.
	50	2	0	49	57	29,90	55		W	1	Cloudy.
8	40	7	0	40	54	29 84	66	0,295	SW	1	Rain.
	42	2	0	42	55	29,77	65		SW	1	Rain.
9	32	7	0	32	53	29,74	63	0,155	WSW	1	Fine.
	40	2	0	40	54	29,83	58		NW	1	Fair.
10	29	7	0	29	51	30,00	61		W	1	Fine.
	35	2	0	35	53	30,05	56		NW	1	Fine.
11	28	7	0	28	49	29,97	62		W	1	Cloudy.
	41	2	0	41	52	29,77	63		W	1	Cloudy.
12	30	7	0	31	48	29,23	72	0,175	NE	2	Snow. [much wind last night.
	31	2	0	31	51	29,52	70		NE	2	Snow.
13	24	7	0	29	47	29,66	63		NE	2	Snow.
	33	2	0	33	50	29.80	61		N	1	Fair.
14	24	7	0	25	47	29,97	64		NE	1	Cloudy.
	31	2	0	31	51	30,08	62		N	1	Fine.
15	18	7	0	20	45	30,18	64		W	1	Fair.
	38	2	0	33	48	30.02	65		SSW	1	Snow.
16	33	7	0	34	47	29,87	70		WNW	1	Fair.
	42	2	0	42	50	29,87	63		NW	1	Fair.

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for February, 1808.

1808	Six's Therm. cast and greatest Heat. °	Time. H.	Time. M.	Therm. without. °	Therm. with. °	Barom. Inches.	Hygro-meter. °	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Feb. 17	30	7	0	32	47	30,04	66		NE	1	Cloudy.
	40	2	0	40	49	30.08	62		W	1	Cloudy.
18	38	7	0	43	48	29,98	73		W	1	Cloudy.
	47	2	0	45	51	30,00	72		NW	1	Cloudy.
19	35	7	0	35	49	30,21	64		NNE	1	Fair.
	42	2	0	42	52	30,29	60		NE	1	Fair.
20	31	7	0	31	48	30,46	65		ENE	1	Fair.
	39	2	0	39	52	30,48	60		E	1	Hazy.
21	29	7	0	29	48	30,52	66		NE	1	Fine.
	40	2	0	39	52	30,52	62		NE	1	Fine.
22	31	7	0	31	48	30,46	67		NE	1	Fine.
	41	2	0	40	53	30,47	62		NE	1	Fair.
23	33	7	0	33	49	30,41	66		NE	1	Cloudy.
	39	2	0	39	51	30,36	63		NE	1	Cloudy.
24	35	7	0	35	49	30,44	65		NE	2	Rain.
	41	2	0	41	52	30,51	58		NE	2	Fair.
25	31	7	0	31	48	30,71	58		NE	2	Fair.
	38	2	0	37	51	30,72	53		NE	1	Fair.
26	29	7	0	31	48	30,64	57		WNW	1	Cloudy.
	38	2	0	37	52	30,50	57		NW	1	Fair.
27	38	7	0	40	50	30,40	6-		NW	1	Cloudy.
	48	2	0	48	54	30,38	56		W	1	Fair.
28	35	7	0	35	51	30,38	66		WSW	1	Fair.
	48	2	0	48	54	30,35	58		WNW	1	Fair.
29	46	7	0	47	52	30,12	66		WNW	1	Cloudy.
	52	2	0	52	55	30,14	65		NW	1	Cloudy.

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1808	Sun's extreme heat and greatest heat	Time H.	Time M.	Therm. w l out °	The m vt in °	P om Inches	Hygrometer. °	Rain. Inches	Winds Points	Str	Weather.
Mar. 1	47	7	0	47	54	30,33	71		NW	1	Cloudy.
	52	2	0	52	50	30,38	60		N	1	Cloudy.
2	47	7	0	47	55	30,34	63		NW	1	Cloudy.
	52	2	0	52	57	30,34	57		NW	1	Cloudy.
3	46	7	0	46	56	30,36	66		NW	1	Cloudy.
	51	2	0	51	57	30,35	54		NNW	1	Cloudy.
4	37	7	0	37	56	30,44	64		NNE	1	Fair.
	54	2	0	54	60	30 46	56		NE	1	Fair.
5	37	7	0	37	56	30,40	65		E	1	Cloudy.
	44	2	0	44	58	30,44	60		E	1	Fair.
6	32	7	0	32	54	30,45	64		NE	1	Fine.
	47	2	0	46	57	30,42	56		NNE	2	Fair.
7	35	7	0	35	53	30,38	60		NE	2	Cloudy.
	43	2	0	43	55	30,32	52		NE	2	Fair
8	34	7	0	35	53	30,28	60		ENE	2	Cloudy.
	40	2	0	39	56	30,28	57		ENE	2	Fine.
9	33	7	0	33	53	30,34	60		NE	2	Cloudy.
	43	2	0	43	56	30,34	56		NE	1	Fair
10	37	7	0	37	52	30,34	66		NE	1	Cloudy.
	40	2	0	40	53	30,38	60		ENE	1	Cloudy.
11	32	7	0	34	52	30,40	60		ENE	1	Fair.
	44	2	0	44	55	30,34	66		NE	1	Cloudy.
12	35	7	0	35	52	30,30	65		NE	1	Cloudy.
	40	2	0	40	54	30,26	60		NE	1	Cloudy.
13	38	7	0	38	52	30,18	66		NE	1	Cloudy.
	43	2	0	43	54	30,17	58		NE	1	Cloudy.
14	37	7	0	37	52	30,12	63		NE	1	Cloudy.
	42	2	0	42	54	30,07	60		ENE	1	Cloudy.
15	36	7	0	37	51	30,00	60		ENE	1	Cloudy.
	44	2	0	44	55	29,97	57		E	1	Fine.
16	35	7	0	36	52	30,00	65		E	1	Cloudy.
	44	2	0	43	55	29,98	56		E	1	Fair

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1808	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Mar.17	31	7	0	33	51	30,00	60		NE	2	Cloudy.
	37	2	0	36	54	30,03	54		NE	1	Fair.
18	31	7	0	33	51	29,98	63		NE	2	Snow.
	37	2	0	35	53	29,90	60		NE	2	Cloudy.
19	30	7	0	33	51	29,76	60		ESE	2	Snow.
	38	2	0	38	53	29,65	58		ESE	2	Cloudy.
20	37	7	0	40	51	29,51	71	0,135	E	1	Cloudy.
	47	2	0	47	53	29,57	73		E	1	Cloudy.
21	39	7	0	39	51	29,73	73	0,032	E	1	Rain.
	42	2	0	42	53	29,80	70		NE	1	Cloudy.
22	38	7	0	38	51	29,93	65		NE	1	Cloudy.
	44	2	0	43	54	29,96	57		NE	2	Hazy.
23	30	7	0	31	50	29,98	62		NE	2	Fair.
	41	2	0	40	54	29,96	53		NE	2	Fair.
24	34	7	0	35	50	29,94	61		NE	2	Sleet.
	41	2	0	39	52	29,91	52		NE	1	Cloudy.
25	26	7	0	27	50	29,97	62		NE	1	Fair.
	40	2	0	40	53	30,06	55		NE	1	Fine.
26	31	7	0	33	51	29,98	67		NE	1	Cloudy.
	46	2	0	45	54	30,00	63		E	1	Cloudy.
27	37	7	0	37	52	30,07	62		NE	2	Cloudy.
	39	2	0	37	53	30,10	58		NE	2	Cloudy.
28	34	7	0	36	50	30,10	60		ENE	1	Fair.
	40	2	0	40	54	30,09	47		NE	2	Fair.
29	29	7	0	31	50	30,14	58		NE	1	Fair.
	43	2	0	43	54	30,13	48		ENE	1	Fine.
30	32	7	0	33	51	30,04	55		NE	2	Cloudy.
	40	2	0	39	52	30,01	51		NE	1	Cloudy.
31	35	7	0	35	50	29,96	63		NNE	2	Sleet.
	43	2	0	43	53	29,93	53		N	1	Cloudy.

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1808	Six's Therm. least and greatest Heat.	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °	Rain. Inches.	Winds. Points.	Str.	Weather.
Apr. 1	32	7 0	33	50	29,78	63		NW	1	Fair.
	43	2 0	42	53	29,65	50		NW	1	Cloudy.
2	27	7 0	29	50	29,84	58		N	2	Fair.
	44	2 0	44	53	29,92	46		NW	1	Fair.
3	30	7 0	33	50	29,90	61		SW	2	Snow.
	50	2 0	48	53	29,83	62		SW	2	Cloudy.
4	40	7 0	42	50	29,72	60		S	2	Cloudy.
	49	2 0	47	52	29,55	66		S	2	Rain.
5	47	7 0	52	53	29,37	72	0,220	SSW	2	Cloudy.
	55	2 0	55	56	29,22	68		SSW	2	Rain.
6	49	7 0	52	54	29,62	73	0,185	SSW	2	Rain.
	56	2 0	56	57	29,69	08		S	2	Cloudy.
7	51	7 0	52	56	29,82	72	0,025	SSW	2	Rain.
	57	2 0	56	58	29,87	06		SSW	2	Cloudy.
8	41	7 0	41	57	29,63	67	0,040	NW	2	Rain.
	52	2 0	51	58	29,88	57		NW	2	Fair.
9	37	7 0	40	56	30,28	63		W	1	Fair.
	53	2 0	53	58	30,28	51		W	1	Fair.
10	43	7 0	44	55	30,29	64		WNW	1	Fair.
	56	2 0	56	58	30,28	51		NW	1	Cloudy.
11	47	7 0	48	57	30,26	54		W	1	Cloudy.
	58	2 0	57	58	30,24	48		W	1	Cloudy.
12	45	7 0	47	56	30,07	61		W	1	Fair.
	54	2 0	51	58	30,14	58		N	2	Rain.
13	40	7 0	41	57	30,32	62	0,032	W	1	Cloudy.
	58	2 0	57	59	30,27	51		WNW	1	Fair.
14	43	7 0	45	57	30,22	60		W	1	Fair.
	64	2 0	64	60	30,14	48		W	1	Fair.
15	45	7 0	48	59	30,07	60		W	1	Fair.
	63	2 0	63	62	30,00	50		W	1	Fine.
16	39	7 0	43	59	30,12	54		NNE	2	Fair.
	54	2 0	54	60	30,08	47		NNE	1	Fair.

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1808	Six's Therm least and greatest Heat.	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygro-me-ter. °	Rain. Inches.	Winds. Points.	Str.	Weather.
Apr. 17	35	7 0	38	57	30,20	57		W	1	Fine.
	53	2 0	52	59	30,12	43		W	2	Cloudy.
18	33	7 0	37	56	30,06	56		WNW	2	Fair.
	48	2 0	46	58	29,95	46		NW	2	Cloudy.
19	36	7 0	41	54	29,64	57		S	2	Cloudy.
	45	2 0	37	55	29,56	60		NW	2	Snow.
20	32	7 0	34	53	29,64	60	0,075	SW	1	Hazy.
	49	2 0	47	55	29,58	50		E	1	Cloudy.
21	40	7 0	46	53	29,15	67	0,460	SSE	2	Cloudy.
	48	2 0	42	55	29,37	62	.	WNW	2	Cloudy.
22	36	7 0	37	53	29,45	63		WSW	2	Fair.
	48	2 0	45	55	29,43	60		SW	2	Rain.
23	39	7 0	42	53	29,43	65	0,145	WSW	1	Cloudy.
	50	2 0	48	55	29,46	57		WNW	1	Cloudy.
24	39	7 0	41	53	29,57	66	0,070	NE	1	Cloudy.
	50	2 0	48	56	29,67	60		NE	2	Rain.
25	36	7 0	37	53	29,90	62	0,016	NNE	2	Fair.
	46	2 0	44	54	29,90	65		N	2	Cloudy.
26	38	7 0	40	53	29,94	64		WNW	1	Cloudy.
	47	2 0	46	54	29,91	55		SW	1	Cloudy.
27	38	7 0	40	52	29,95	64		NE	1	Rain.
	45	2 0	43	54	29,91	67		N	1	Rain.
28	39	7 0	41	52	29,89	65	0,378	NE	1	Cloudy.
	48	2 0	48	55	29,90	60		NE	1	Cloudy.
29	38	7 0	40	53	29,94	61		NE	1	Cloudy.
	47	2 0	44	55	29,94	58		NE	1	Cloudy.
30	38	7 0	39	53	29,91	60		E	1	Hazy.
	52	2 0	51	55	29,90	50		S	1	Cloudy.

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1808	Six's Therm least and greatest Heat.	Time. H. M.		Therm. without. °	Therm. within. °	Barom. Inches.	Hygro-me-ter. °	Rain. Inches.	Winds. Points.	Str.	Weather.
May 1	45	7	0	46	54	29,95	60		SW	1	Cloudy.
	62	2	0	61	57	29,98	52		S	1	Fair.
2	43	7	0	47	55	29,98	62		E	1	Fine.
	64	2	0	63	58	29,90	52		E	1	Fair.
3	54	7	0	57	57	29,83	57		E	1	Hazy.
	75	2	0	73	62	29,80	50		E	1	Fine.
4	51	7	0	55	59	29,83	57		E	1	Fair.
	75	2	0	75	64	29,83	47		E	1	Fine.
5	53	7	0	57	62	29,86	53		ENE	1	Hazy.
	72	2	0	70	64	29,77	48		E	1	Cloudy.
6	54	7	0	55	62	29,77	60		SSW	1	Cloudy.
	70	2	0	69	63	29,75	48		SSW	1	Fair.
7	56	7	0	58	63	29,62	58		S	1	Fine.
	69	2	0	69	64	29,56	51		S	2	Fair.
8	52	7	0	52	63	29,57	58	0,055	S	1	Rain.
	60	2	0	58	63	29,60	53		S	1	Cloudy.
9	48	7	0	50	61	29,55	57		SE	2	Cloudy.
	55	2	0	53	61	29,57	58		SSW	2	Rain.
10	46	7	0	48	60	29,80	61	0,230	S	2	Fine.
	60	2	0	60	60	29,75	58		S	2	Cloudy.
11	50	7	0	51	60	29,95	61		S	1	Fair.
	62	2	0	62	61	30,02	52		S	2	Cloudy.
12	49	7	0	52	60	30,17	64	0,070	S	2	Cloudy.
	65	2	0	63	62	30,21	55		S	2	Cloudy.
13	54	7	0	55	62	30,28	65		S	1	Cloudy.
	70	2	0	69	64	30,28	64		S	1	Hazy.
14	58	7	0	59	63	30,19	60		S	1	Cloudy.
	78	2	0	78	65	30,17	48		S	1	Fair.
15	58	7	0	61	64	30,19	60		SW	1	Fine.
	79	2	0	79	69	30,13	46		S	1	Fine.
16	65	7	0	66	67	30,00	50		S	1	Hazy.
	82	2	0	81	69	29,98	47		S	1	Hazy.

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1808	Six's Therm. least and greatest Heat. °	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter. °	Rain. Inches.	Winds. Points.	Str.	Weather.
May 17	59	7	0	62	67	30,10	53		E	1	Hazy.
	75	2	0	75	69	30,05	48		NE	1	Hazy.
18	54	7	0	57	67	30,06	54		NE	1	Cloudy.
	59	2	0	58	63	30,14	54		NE	1	Cloudy.
19	42	7	0	46	64	30,16	56		NE	1	Hazy.
	59	2	0	57	64	30,21	50		ENE	1	Fair.
20	44	7	0	50	62	30,08	58		ENE	1	Fair.
	63	2	0	62	63	29,96	48		E	1	Fair.
21	53	7	0	54	63	29,80	61	0,105	E	1	Rain.
	65	2	0	63	63	29,76	56		SW	1	Cloudy.
22	54	7	0	56	63	29,61	67	0,160	SW	1	Cloudy.
	68	2	0	65	63	29,55	57		SSW	1	Cloudy.
23	53	7	0	54	62	29,65	60	0,185	SW	1	Cloudy.
	65	2	0	64	63	29,72	52		SW	2	Fair.
24	48	7	0	51	61	29,84	60		SW	2	Fine.
	65	2	0	64	62	29,90	50		SW	2	Cloudy.
25	52	7	0	55	61	29,95	54		N	1	Cloudy.
	72	2	0	70	64	29,91	52		ESE	1	Fair.
26	56	7	0	57	63	29,74	64	0,093	E	1	Rain.
	65	2	0	64	64	29,68	65		E	1	Rain.
27	54	7	0	55	63	29,76	62	0,180	S	1	Cloudy.
	63	2	0	61	62	29,80	61		S	1	Rain.
28	50	7	0	52	62	29,98	63	0,045	SSW	2	Fair.
	69	2	0	68	63	30,05	50		SSW	1	Fair.
29	52	7	0	55	62	30,17	58		SSE	1	Cloudy.
	66	2	0	65	62	30,20	58		S	1	Cloudy.
30	55	7	0	57	62	30,22	63		S	1	Fair.
	75	2	0	74	64	30,16	51		S	1	Fair.
31	56	7	0	60	63	30,03	60		NE	1	Hazy.
	76	2	0	74	66	29,87	51		E	1	Hazy.

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1808	Six's Therm. least and greatest Heat.	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °	Rain. Inches.	Winds. Points.	Str.	Weather.
June 1	60	7 0	60	64	29,82	63	0,035	N	1	Cloudy.
	63	2 0	62	64	29,91	56		NW	1	Cloudy.
2	48	7 0	52	63	30,02	60	0,035	WSW	1	Fine.
	69	2 0	69	64	30,00	47		SW	1	Fine.
3	51	7 0	54	62	29,90	57		S	2	Cloudy.
	66	2 0	65	63	29,86	47		S	2	Fair.
4	56	7 0	57	63	29,70	57		ESE	1	Hazy.
	63	2 0	62	63	29,63	58		ESE	1	Rain.
5	52	7 0	55	62	29,68	60	0,043	S	2	Fair.
	65	2 0	64	63	29,68	52		S	2	Cloudy.
6	49	7 0	51	61	29,71	61		SW	1	Cloudy.
	59	2 0	59	60	29,78	61		W	1	Cloudy.
7	50	7 0	52	60	29,83	58		W	1	Cloudy.
	66	2 0	59	61	29,86	53		WSW	1	Cloudy.
8	50	7 0	53	61	29,84	59		WSW	2	Fair.
	65	2 0	65	62	29,85	50		WNW	1	Fair.
9	52	7 0	53	60	29,65	64	0,035	SW	1	Rain.
	60	2 0	58	61	29,65	58		WSW	1	Rain.
10	52	7 0	53	60	29,85	65	0,140	NNE	2	Cloudy.
	63	2 0	62	62	29,94	55		NNE	2	Fair.
11	46	7 0	49	60	30,05	64		SW	1	Hazy.
	72	2 0	72	62	30,04	47		NW	1	Fair.
12	51	7 0	55	61	30,18	58		N	1	Cloudy.
	66	2 0	65	62	30,19	53		NE	1	Cloudy.
13	52	7 0	55	61	30,16	63		SW	1	Fair.
	72	2 0	70	63	30,12	51		W	1	Cloudy.
14	54	7 0	57	62	30,07	57		SW	1	Fair.
	72	2 0	70	64	29,98	50		SSW	2	Fair.
15	56	7 0	58	62	29,90	57	0,062	WNW	1	Fair.
	67	2 0	63	62	29,95	51		NW	1	Cloudy.
16	49	7 0	53	62	30,11	58		WNW	1	Fine.
	66	2 0	65	62	30,13	50		WNW	1	Fair.

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1808	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygro-me-ter.	Rain.	Winds.		Weather.
	°	H.	M.	°	°	Inches.	°	Inches.	Points	Str.	
June 17	55	7	0	57	62	30,15	60		SW	1	Cloudy.
	67	2	0	64	62	30,08	51		SW	1	Cloudy.
18	60	7	0	60	62	30,05	64		SW	1	Cloudy.
	76	2	0	75	65	30,08	55		W	1	Fair.
19	58	7	0	59	64	30,11	62		SW	1	Fair.
	76	2	0	75	66	30,08	50		W	1	Fair.
20	61	7	0	61	65	30,08	60		SW	1	Fine.
	75	2	0	73	66	30,03	50		N	1	Cloudy.
21	58	7	0	60	65	30,01	60		E	1	Cloudy.
	70	2	0	70	67	29,95	55		E	1	Cloudy.
22	57	7	0	58	65	29,83	55		E	1	Fair.
	70 ·	2	0	70	67	29,77	53		E	1	Fine.
23	56	7	0	57	65	29,72	65	0,290	SW	1	Rain.
	69	2	0	68	66	29,77	50		S	2	Fair.
24	53	7	0	56	65	29,90	57		NE	1	Fair.
	71	2	0	70	65	29,91	48		NE	1	Hazy.
25	54	7	0	57	65	30,05	60		SW	1	Fair.
	73	2	0	72	67	30,04	54		NE	1	Fair.
26	54	7	0	57	65	30,10	57		E	1	Fine.
	75	2	0	75	67	30,07	49		NE	1	Fine.
27	56	7	0	56	65	30,08	58		NE	1	Cloudy.
	68	2	0	66	66	30,08	53		NE	1	Fair.
28	53	7	0	54	63	30,11	57		NE	1	Cloudy.
	61	2	0	60	64	30,11	55		NE	1	Cloudy.
29	54	7	0	56	64	30,17	59		NE	1	Cloudy.
	75	2	0	72	66	30,17	51		E	1	Fine.
30	54	7	0	57	65	30,26	60		NE	1	Cloudy.
	70	2	0	70	67	30,25	53		ENE	2	Fair.

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1808	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.	°	Inches.	Points.	Str	
July 1	52	7	0	57	65	30,20	57		NE	1	Cloudy.
	70	2	0	70	67	30,13	52		E	1	Fine.
2	51	7	0	57	63	30,10	58		ENE	2	Fair.
	69	2	0	68	67	30,05	51		NE	1	Fair.
3	54	7	0	57	64	30,05	57		NE	1	Fine.
	71	2	0	71	66	30,01	50		NE	1	Fair.
4	50	7	0	54	64	30,04	63	0,080	NE	1	Hazy.
	70	2	0	69	66	29,97	47		W	1	Fair.
5	50	7	0	54	64	29,99	57		NE	1	Fine.
	67	2	0	67	64	30,03	47		NE	1	Fair.
6	55	7	0	59	63	30,12	58		WNW	1	Cloudy.
	69	2	0	67	64	30,18	54		WNW	1	Cloudy.
7	58	7	0	58	64	30,19	57		SSW	1	Fine.
	75	2	0	75	66	30,13	48		S	1	Fine.
8	56	7	0	59	64	30,07	55		SSW	2	Fine.
	77	2	0	77	67	30,00	47		S	2	Fair.
9	60	7	0	62	65	30,04	64		W	1	Cloudy.
	72	2	0	71	67	30,10	56		N	1	Cloudy.
10	58	7	0	59	65	30,10	62		S	1	Fair
	74	2	0	73	67	30,10	53		S	2	Cloudy.
11	60	7	0	61	66	30,18	65		SW	1	Cloudy.
	80	2	0	78	68	30,18	50		S	1	Fine.
12	62	7	0	67	68	30,17	61		NE	1	Hazy.
	90	2	0	88	73	30,12	45		SE	1	Hazy.
13	67	7	0	72	72	30,03	55		NE	1	Fine.
	93½	2	0	92	76	30,00	45		S	1	Fine.
14	71	7	0	73	75	30,04	54		SW	1	Hazy.
	91	2	0	90	78	30,07	45		W	1	Hazy.
15	65	7	0	70	75	30,04	58		NE	2	Fine.
	79	2	0	78	77	30,00	54		E	2	Fine.
16	64	7	0	68	74	29,97	59		E	2	Fine.
	86	2	0	85	77	30,05	52		S	1	Hazy

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1808	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
July 17	61	7	0	64	73	30,04	63		SW	1	Cloudy.
	83	2	0	82	75	30,99	48		NNE	1	Fair.
18	64	7	0	67	75	30,13	56		N	1	Fine.
	83	2	0	83	76	30,05	47		E	1	Hazy.
19	64	7	0	68	74	29,98	56		E	1	Hazy.
	88	2	0	84	77	29,85	50		E	1	Fair.
20	61	7	0	63	72	29,86	57		SSW	1	Fine.
	75	2	0	75	73	29,81	50		S	2	Cloudy.
21	59	7	0	60	70	29,74	60	0,016	S	1	Fair.
	76	2	0	75	72	29,71	50		S	2	Fair.
22	59	7	0	60	68	29,76	60		SE	2	Cloudy.
	74	2	0	72	69	29,79	50		S	2	Fair.
23	63	7	0	64	69	29,84	57		E	1	Fine.
	78	2	0	77	72	29,84	50		SE	2	Fair.
24	64	7	0	64	70	29,84	60	0,046	S	1	Fine.
	76	2	0	70	71	29,78	61		NW	1	Rain.
25	62	7	0	65	70	29,76	62	0,440	W	1	Cloudy.
	72	2	0	69	71	29,76	60		W	1	Rain. {thunder
26	60	7	0	63	69	29,82	66	0,430	SW	1	Cloudy.
	73	2	0	72	70	29,84	56		W	1	Cloudy.
27	60	7	0	62	68	29,85	64	0,190	NE	1	Cloudy.
	75	2	0	74	70	29,77	64		ESE	1	Cloudy.
28	59	7	0	61	68	29,48	77	0,943	N	1	Rain.
	66	2	0	66	69	29,51	69		NW	1	Rain.
29	62	7	0	63	68	29,65	70	0,313	WSW	1	Cloudy.
	75	2	0	73	69	29,70	53		S	2	Cloudy.
30	62	7	0	63	68	29,74	65		S	1	Cloudy.
	76	2	0	76	70	29,80	50		S	1	Fair.
31	60	7	0	62	68	29,85	62		S	1	Fine.
	80	2	0	80	71	29,74	50		ESE	1	Fair.

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1808	Six's Therm. least and greatest Heat.	Time. H. M.	Therm without °	Therm. within °	Barom Inches	Hygrometer °	Rain Inches	Winds Points	Str.	Weather.
Aug. 1	62	7 0	64	68	29,64	64	0,352	S	2	Fair.
	71	2 0	69	69	29 57	61		S	2	Rain.
2	59	7 0	60	69	29,80	67	0,160	SW	1	Cloudy.
	76	2 0	75	70	29,88	52		NW	2	Fair.
3	59	7 0	61	67	30,09	56		WSW	1	Cloudy.
	72	2 0	72	69	30,14	51		W	1	Cloudy.
4	58	7 0	59	67	30,1o	63		SW	1	Fine.
	77	2 0	77	70	30,02	48		S	1	Fine.
5	58	7 0	59	68	29,91	60		S	1	Fine.
	80	2 0	78	70	29,82	48		S	1	Fine.
6	64	7 0	65	69	29,74	60		S	1	Fair
	77	2 0	75	71	29,74	52		S	2	Cloudy.
7	61	7 0	62	68	29,85	63		S	2	Cloudy.
	71	2 0	70	69	29 88	57		S	2	Cloudy.
8	63	7 0	63	68	29,83	70	0,295	S	1	Rain.
	75	2 0	75	70	29,76	51		S	2	Fair.
9	61	7 0	61	68	29,63	64	0,182	E	1	Cloudy.
	72	2 0	71	69	29,59	55		E	1	Cloudy.
10	59	7 0	62	68	29,77	65	0,016	W	1	Cloudy.
	73	2 0	72	69	29,78	53		W	1	Fair.
11	60	7 0	62	67	29,74	59		SW	1	Cloudy.
	74	2 0	74	69	29,72	48		S	1	Fair.
12	58	7 0	58	66	29,81	59		S	1	Hazy.
	76	2 0	76	70	29,83	49		S	1	Fine.
13	61	7 0	63	67	29,75	60		S	2	Cloudy.
	69	2 0	68	68	29,67	64		S	2	Rain.
14	60	7 0	61	67	29,72	64	0,045	S	1	Rain.
	73	2 0	72	68	29,66	51		W	2	Cloudy.
15	58	7 0	60	65	29 64	62		SSW	2	Fair.
	73	2 0	73	68	29 68	50		W	2	Fair.
16	55	7 0	58	65	29,87	61	0,052	SW	2	Cloudy.
	72	2 0	72	68	29,84	52		SW	1	Cloudy.

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1808	Six's Therm. least and greatest Heat.	Time.		Therm. without	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	°	H.	M.	°	°	Inches.	°	Inches.	Points	Str.	
Aug.17	60	7	0	61	66	29,88	60		WSW	1	Cloudy.
	71	2	0	70	67	29,95	50		W	1	Cloudy.
18	59	7	0	61	65	30,04	54		E	1	Hazy.
	71	2	0	71	67	30,05	50		NE	1	Fine.
19	60	7	0	62	66	30,11	58		W	1	Cloudy.
	72	2	0	72	67	30,13	48		NW	1	Cloudy.
20	56	7	0	58	65	30,18	61		N	1	Hazy.
	72	2	0	72	68	30,19	50		E	1	Fine.
21	58	7	0	60	66	30,21	62		E	1	Fine.
	76	2	0	76	68	30,21	53		ENE	1	Fair.
22	60	7	0	62	66	30,20	62		NE	1	Fair.
	72	2	0	71	68	30,18	57		E	1	Fair.
23	56	7	0	60	65	30,17	58		NE	1	Hazy.
	69	2	0	69	67	30,15	52		ENE	2	Cloudy.
24	57	7	0	59	65	30,12	57		NE	1	Fine.
	71	2	0	71	67	30,12	50		NE	1	Fair.
25	55	7	0	58	64	30,12	66		NE	1	Hazy.
	71	2	0	71	67	30,05	50		NE	1	Fair.
26	55	7	0	56	65	29,85	63		SW	1	Fine.
	73	2	0	73	68	29,71	50		SW	1	Fair.
27	58	7	0	60	65	29,56	64		SW	2	Cloudy.
	68	2	0	67	66	29,62	50		NW	1	Cloudy.
28	51	7	0	53	63	29,68	60	0,020	SW	1	Fine.
	68	2	0	68	66	29,77	45		WSW	1	Fine.
29	52	7	0	55	64	29,91	60		SW	1	Fine.
	71	2	0	71	67	29,90	50		SE	1	Fair.
30	61	7	0	64	67	29,75	66	0,018	S	2	Cloudy.
	74	2	0	73	68	29,68	50		S	2	Cloudy.
31	57	7	0	58	65	29,61	65	0,075	S	1	Fine.
	68	2	0	66	66	29,55	58		S	2	Rain.

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1808	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °	Rain. Inches.	Winds. Points.	Str.	Weather.
Sep. 1	57	7	0	58	65	29,59	63	0,095	S	2	Fine. ⎡much wind ⎣last night.
	69	2	0	68	67	29,66	57		S	2	Cloudy.
2	55	7	0	56	65	29,82	63	0,245	S	2	Fair.
	68	2	0	64	64	29,83	63		S	1	Rain.
3	55	7	0	56	64	29,84	66	0,395	SW	1	Cloudy.
	68	2	0	67	66	29,86	55		WNW	1	Cloudy.
4	57	7	0	58	64	29,83	69	0,045	SW	1	Cloudy.
	67	2	0	66	66	29,85	63		SW	1	Cloudy.
5	52	7	0	54	63	29,82	62		S	1	Cloudy.
	66	2	0	64	64	29,72	60		S	2	Cloudy.
6	53	7	0	54	65	29,72	63	0,073	S	2	Fair.
	67	2	0	66	65	29,72	53		SW	1	Fair.
7	54	7	0	56	63	29,78	67	0,063	S	2	Cloudy.
	67	2	0	67	64	29,76	60		S	2	Cloudy.
8	57	7	0	58	63	29,58	64	0,055	S	1	Cloudy.
	67	2	0	67	64	29,51	53		S	2	Fair.
9	53	7	0	56	63	29,31	62		SE	2	Cloudy.
	64	2	0	63	64	29,30	60		S	2	Fair.
10	56	7	0	57	63	29,32	65	0,373	ESE	1	Rain.
	63	2	0	62	64	29,33	65		SE	1	Rain.
11	54	7	0	54	63	29,44	69	0,570	SW	1	Cloudy.
	64	2	0	59	63	29,53	63		W	1	Rain.
12	54	7	0	54	62	29,63	70	0,586	SW	1	Cloudy.
	64	2	0	64	63	29,65	64		SW	1	Rain.
13	54	7	0	56	62	29,69	72	0,480	NE	1	Rain.
	65	2	0	64	63	29,71	60		SE	1	Rain.
14	55	7	0	57	62	29,76	73	0,285	NW	1	Cloudy.
	66	2	0	66	64	29,82	59		NE	1	Fair.
15	58	7	0	60	63	30,00	71		NNE	1	Cloudy.
	71	2	0	71	65	30,11	53		NE	1	Fair.
16	56	7	0	56	63	30,27	70		NE	1	Cloudy.
	66	2	0	65	65	30,28	51		E	1	Fair.

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1808	Six's Therm. least and greatest Heat.	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygro-me-ter. °	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Sep. 17	52	7 0	53	63	30,24	63		NE	1	Fine.
	65	2 0	65	64	30,16	51		NE	2	Fair.
18	54	7 0	54	63	29,96	68		ENE	1	Cloudy.
	68	2 0	68	65	29,88	55	0,180	E	1	Fair.
19	59	7 0	59	63	29,86	73		NW	1	Rain.
	67	2 0	66	66	29,93	53		W	1	Fair.
20	52	7 0	53	63	30,18	67		SW	1	Fine.
	68	2 0	66	65	30,25	53		W	1	Fair.
21	51	7 0	52	64	30,34	67		W	1	Foggy.
	66	2 0	66	65	30,31	58		S	1	Fine.
22	51	7 0	52	64	30,16	66		S	1	Foggy.
	67	2 0	67	64	29,97	57		S	1	Cloudy.
23	53	7 0	53	63	29,64	65	0,335	W	1	Fair.
	59	2 0	58	63	29,66	63		N	1	Cloudy.
24	45	7 0	46	61	29,94	62		N	1	Fine.
	55	2 0	54	62	29,98	53		N	1	Fair.
25	49	7 0	51	61	30,05	61		WSW	1	Cloudy.
	60	2 0	59	61	30,09	60		NE	1	Cloudy.
26	48	7 0	49	60	30,08	62		E	1	Fine.
	63	2 0	63	63	30,01	56		SSW	1	Fine.
27	48	7 0	48	61	29,94	56		NW	1	Cloudy.
	56	2 0	56	62	29,88	50		N	1	Fine.
28	42	7 0	44	60	29,76	60		WNW	1	Cloudy.
	52	2 0	52	60	29,57	51		SW	1	Hazy.
29	40	7 0	40	57	29,40	56		N	1	Cloudy.
	52	2 0	50	58	29,40	57		N	1	Rain.
30	39	7 0	39	57	29,51	63		NW	1	Cloudy.
	52	2 0	51	60	29,58	56		NW	1	Fair.

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1808	Six's Therm. least and greatest Heat.	Time. H. M.		Therm w thout °	Therm. within °	Barom Inches	Hygrometer.	Rain. Inches.	Winds. Points	Str.	Weather.
Oct. 1	41	7	0	41	57	29,85	60	0,053	NW	1	Fair.
	52	2	0	52	57	29,91	50		NW	1	Fair.
2	39	7	0	43	56	29,82	60		SW	1	Cloudy.
	56	2	0	54	58	29,61	67		SE	2	Rain.
3	48	7	0	48	57	29,90	71	0,158	W	1	Rain.
	56	2	0	56	59	30,06	60		W	1	Cloudy.
4	45	7	0	46	57	30,19	66		WSW	1	Fair.
	57	2	0	57	59	30,21	66		SW	1	Rain.
5	48	7	0	48	57	30,20	71	0,122	SW	1	Foggy.
	62	2	0	61	60	30,05	60		S	1	Cloudy.
6	46	7	0	48	57	30,02	69		E	1	Fine.
	63	2	0	62	60	30,01	61		WSW	1	Fair.
7	50	7	0	51	58	30,06	63		N	1	Cloudy.
	57	2	0	56	60	29,91	62		SSW	1	Cloudy.
8	48	7	0	48	58	29,34	62	0,365	W	2	Fine.
	53	2	0	52	59	29,36	53		WNW	2	Cloudy.
9	41	7	0	41	57	29,73	60	0,016	WNW	2	Fine.
	52	2	0	51	58	29 82	50		NW	2	Fine.
10	43	7	0	49	57	29,74	68		W	1	Cloudy.
	58	2	0	57	59	29,85	54		NW	2	Cloudy.
11	43	7	0	43	57	30,04	65		W	1	Fair.
	58	2	0	57	60	29,96	56		SW	1	Fine.
12	45	7	0	45	58	29,95	62	0,133	W	1	Fine.
	53	2	0	52	59	29,97	56		WNW	1	Fair.
13	38	7	0	38	57	30,07	58		NW	1	Fine.
	48	2	0	48	59	30,11	50		NW	1	Fair.
14	44	7	0	47	56	29,59	67	0,057	S	2	Rain.
	57	2	0	54	59	29,39	53		WNW	2	Cloudy.
15	40	7	0	43	56	29,18	65	0,065	SW	1	Rain.
	50	2	0	50	57	29, 20	60		W	1	Rain.
16	43	7	0	43	56	29,55	65	0,061	W	1	Cloudy.
	51	2	0	49	56	29,53	60		SW	1	Rain.

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1808	Six's Therm. least and greatest Heat. °	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °	Rain. Inches.	Winds. Points.	Str.	Weather.
Oct. 17	41	7	0	41	54	29,56	62	0,047	W	1	Fair.
	48	2	0	47	56	29,61	55		NW	2	Fair.
18	40	7	0	41	54	29,77	64		WSW	1	Fair.
	53	2	0	52	57	29,72	55		W	1	Fair.
19	40	7	0	41	54	29,50	63	0,072	SW	1	Fine.
	50	2	0	50	55	29,48	55		W	1	Cloudy.
20	43	7	0	43	53	29,69	63		SW	1	Fair.
	53	2	0	53	56	29,73	54		NW	1	Cloudy.
21	47	7	0	52	55	29,30	73	0,305	SSW	1	Rain.
	53	2	0	52	59	29,48	53		WNW	2	Fair.
22	42	7	0	44	55	29,53	63		SW	1	Fair.
	50	2	0	50	57	29,57	53		W	1	Fine.
23	35	7	0	35	54	29,81	63		SW	1	Fine.
	52	2	0	51	56	29,78	56		SSW	1	Hazy.
24	48	7	0	48	55	29,32	75	0,465	SW	1	Rain. [much win last night
	55	2	0	54	57	29,43	55		SW	2	Cloudy.
25	40	7	0	40	54	29,71	68		SW	1	Fine.
	54	2	0	53	56	29,61	59		S	2	Hazy.
26	39	7	0	39	56	29,22	67	0,335	SW	1	Cloudy.
	56	2	0	54	58	29,28	56		SW	2	Fair.
27	43	7	0	44	55	29,42	64		S	2	Cloudy.
	52	2	0	51	57	29,45	61		S	2	Rain.
28	44	7	0	48	56	29,41	70	0,515	S	2	Rain.
	53	2	0	53	58	29,48	62		S	2	Fair.
29	46	7	0	46	56	29,67	68	0,318	SE	1	Cloudy.
	52	2	0	52	58	29,76	70		S	1	Cloudy.
30	44	7	0	44	56	30,03	70	0,112	S	1	Rain.
	53	2	0	52	57	30,11	67		NE	1	Cloudy.
31	45	7	0	45	56	30,31	73		NE	1	Cloudy.
	•52	2	0	52	58	30,24	67		NE	1	Cloudy.

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1808	Six's Therm. least and greatest Heat.	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °	Rain. Inches.	Winds. Points.	Str.	Weather.
Nov. 1	48	7 0	48	56	30,29	68		NE	1	Cloudy.
	52	2 0	51	58	30,24	63		NE	1	Cloudy.
2	48	7 0	48	56	30,18	65		ENE	1	Cloudy.
	52	2 0	52	58	30,13	60		ENE	1	Cloudy.
3	48	7 0	48	56	30,05	68		ENE	2	Cloudy.
	49	2 0	48	58	30,07	63		E	2	Cloudy.
4	44	7 0	44	56	30,22	58		E	1	Fair.
	48	2 0	48	58	30,18	56		E	1	Cloudy.
5	40	7 0	40	55	29,91	60		NE	1	Cloudy.
	44	2 0	43	56	29,89	57		ESE	1	Fair.
6	31	7 0	32	53	29,92	62		NE	1	Fine.
	44	2 0	44	56	29,82	60		E	1	Fine.
7	38	7 0	41	53	29,72	68		ENE	1	Cloudy.
	46	2 0	46	55	29,68	63		E	1	Cloudy.
8	44	7 0	44	53	29,66	70		NE	1	Cloudy.
	51	2 0	51	55	29,62	70		NE	1	Cloudy.
9	46	7 0	46	54	29,56	73	0,085	N	1	Cloudy.
	53	2 0	53	55	29,63	72		NE	1	Cloudy.
10	47	7 0	47	55	29,76	73		NE	1	Fair.
	52	2 0	52	58	29,73	65		NE	1	Fine.
11	47	7 0	47	57	29,85	62		NE	1	Cloudy.
	49	2 0	49	58	29,88	64		NE	1	Cloudy.
12	38	7 0	38	56	30,07	63		NE	1	Fair.
	45	2 0	44	56	30,12	57		E	1	Cloudy.
13	40	7 0	40	55	30,12	60		ENE	1	Cloudy.
	43	2 0	43	55	30,10	58		NE	1	Cloudy.
14	33	7 0	33	55	30,12	60		NE	1	Fine.
	38	2 0	38	54	30,11	60		NE	1	Hazy.
15	34	7 0	35	51	30,00	63		SW	1	Cloudy.
	47	2 0	47	53	29,92	65		S	1	Cloudy.
16	46	7 0	50	52	29,67	72		S	2	Cloudy.
	52	2 0	51	55	29,50	63		S	2	Fair.

METEOROLOGICAL JOURNAL

for November, 1808.

1808	Six's Therm. least and greatest Heat. °	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer °	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Nov.17	50	7	0	52	54	29,31	73	0,150	S	2	Cloudy.
	56	2	0	55	58	29,20	65		S	2	Fair.
18	48	7	0	48	56	28,80	70	0,095	S	2	Rain.
	50	2	0	50	57	28,74	64		S	2	Cloudy.
19	36	7	0	37	54	29,25	67	0,440	NE	1	Snow.
	44	2	0	44	56	29,54	60		WNW	1	Fair.
20	39	7	0	40	54	29,75	65		WSW	1	Cloudy.
	48	2	0	48	56	29,78	66		SSW	1	Cloudy.
21	36	7	0	52	55	29,75	71		SSW	2	Cloudy.
	56	2	0	54	57	29,85	58		WSW	1	Fair.
22	41	7	0	41	54	30,18	66		W	1	Fair.
	49	2	0	49	55	30,22	62		W	1	Cloudy.
23	47	7	0	48	54	30,13	70		W	1	Cloudy.
	54	2	0	54	57	30,13	66		SW	1	Cloudy.
24	47	7	0	47	55	30,18	64		N	1	Cloudy.
	50	2	0	49	57	30,22	57		N	1	Cloudy.
25	46	7	0	48	56	30,02	73	0,415	SW	1	Rain.
	55	2	0	55	59	29,95	69		SW	1	Cloudy.
26	46	7	0	48	55	29,98	68		WSW	1	Cloudy.
	56	2	0	56	59	29,89	67		SW	1	Cloudy.
27	52	7	0	52	56	29,55	72	0,060	SW	1	Cloudy.
	56	2	0	48	58	29,45	66		NW	1	Rain.
28	36	7	0	36	56	29,78	64		N	1	Fair.
	42	2	0	42	57	29 86	62		NW	1	Fine.
29	35	7	0	35	53	29,81	65		NW	1	Cloudy.
	42	2	0	38	55	29,72	65		S	1	Rain.
30	39	7	0	43	54	29,13	66	0,500	W	1	Fair.
	49	2	0	48	55	29,30	57		WNW	1	Fair.

METEOROLOGICAL JOURNAL

for December, 1808.

1808	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	°Heat.	H.	M.	°	°	Inches.	°	Inches.	Points.	str.	
Dec. 1	39	8	0	39	53	29,37	68		W	1	Fair.
	48	2	0	48	55	29,48	62		WSW	1	Fine.
2	44	8	0	44	55	29,20	66	0,175	W	2	Fair.
	50	2	0	50	56	29,24	61		W	2	Cloudy.
3	43	8	0	44	55	29,48	66		WSW	1	Fair.
	50	2	0	50	57	29,44	60		SW	1	Fine.
4	45	8	0	45	55	30,02	65		N	1	Cloudy.
	47	2	0	46	57	30,16	61		N	1	Fair.
5	36	8	0	40	53	30,30	63		NE	1	Cloudy.
	52	2	0	49	56	30,28	67		S	1	Cloudy.
6	49	8	0	49	55	30,02	71		SW	2	Cloudy.
	53	2	0	53	57	39,78	64		SW	2	Cloudy.
7	39	8	0	39	53	29,86	57	0,020	WNW	1	Fair.
	43	2	0	43	55	29,60	55		N	1	Fair.
8	38	8	0	40	52	29,95	55		WNW	2	Cloudy.
	43	2	0	43	54	29,99	56		NW	1	Cloudy.
9	41	8	0	42	53	29,88	64		N	1	Cloudy.
	45	2	0	44	56	29,94	62		NNE	1	Cloudy.
10	34	8	0	36	53	30,08	63		N	1	Cloudy.
	42	2	0	41	55	30,13	63		S	1	Fair.
11	37	8	0	37	53	30,30	66		WNW	1	Fair.
	42	2	0	42	54	30,30	68		NW	1	Cloudy.
12	34	8	0	38	52	30,28	68		WNW	1	Foggy.
	43	2	0	42	53	30,30	68		NW	1	Cloudy.
13	38	8	0	38	51	30,33	70		N	1	Cloudy.
	43	2	0	43	53	30,35	70		NW	1	Cloudy.
14	34	8	0	35	50	30,35	70		W	1	Cloudy.
	43	2	0	41	52	30,25	67		WSW	1	Cloudy.
15	37	8	0	38	50	30,07	66	0,045	NW	1	Cloudy.
	41	2	0	41	52	30,05	64		NNE	1	Fair.
16	35	8	0	36	50	30,10	70		NNE	1	Cloudy.
	37	2	0	37	52	30,06	60		NE	1	Fair.

METEOROLOGICAL JOURNAL

for December, 1808.

1808	Six's Therm. least and greatest Heat. °	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °	Rain. Inches.	Winds. Points.	Str.	Weather.
Dec. 17	32	8	0	33	50	29,86	67		NW	1	Foggy.
	40	2	0	39	52	29,52	67		W	1	Sleet.
18	24	8	0	25	46	29,63	60		N	2	Cloudy.
	32	2	0	30	50	29,65	60		N	2	Fine.
19	29	8	0	30	46	29,53	68		N	1	Cloudy.
	34	2	0	32	49	29,68	67		N	1	Snow.
20	27	8	0	27	45	29,67	67		N	1	Cloudy.
	31	2	0	31	48	29,77	65		N	1	Fair.
21	19	8	0	20	44	30,00	65		W	1	Fair.
	35	2	0	31	46	29,85	63		SSW	1	Cloudy.
22	31	8	0	31	43	29,24	68		W	1	Snow.
	34	2	0	34	45	29,07	69		W	1	Cloudy.
23	27	8	0	28	44	29,40	64		N	2	Cloudy.
	33	2	0	31	47	29,41	65		N	2	Cloudy.
24	30	8	0	31	46	29,46	70		NNE	1	Cloudy.
	33	2	0	30	47	29,55	67		E	1	Cloudy.
25	27	8	0	29	45	29,60	67		NNE	1	Cloudy.
	31	2	0	31	46	29,50	67		NNE	1	Foggy.
26	23	8	0	23	43	29,51	67		NE	1	Fair.
	31	2	0	31	46	29,50	66		ENE	1	Cloudy.
27	31	8	0	34	43	29,47	70		NE	1	Cloudy.
	36	2	0	36	46	29,48	72		NE	1	Cloudy.
28	36	8	0	38	43	29,60	73		NE	1	Cloudy.
	40	2	0	39	47	29,58	73		NE	1	Cloudy.
29	37	8	0	39	46	29,50	75	0,450	E	1	Rain.
	43	2	0	43	48	29,48	75		E	1	Cloudy.
30	39	8	0	40	47	29,49	74	0,043	E	1	Cloudy.
	45	2	0	44	50	29,50	74		E	1	Cloudy.
31	38	8	0	38	47	29,59	76		E	1	Cloudy.
	38	2	0	38	50	29,60	71		E	1	Cloudy.

1808.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	53	18	38,8	52	18	39,2	56	46	51,1	30,51	28,90	29,84	74	53	65,1	1,059
February	53	18	38,3	52	20	38,5	57	45	51,4	30,72	29,23	30,10	73	53	63,3	0,710
March	52	26	39,2	52	27	39,4	60	50	53,1	30,48	29,51	30,12	73	47	60,4	0,167
April	64	27	45,2	64	29	45,6	62	50	54,7	30,29	29,15	29,86	73	43	59,2	1,646
May	82	42	60,0	81	46	60,6	69	54	62,5	30,28	29,55	29,93	65	46	55,9	1,123
June	76	48	61,1	75	49	61,4	67	60	63,4	30,26	29,63	29,97	65	47	56,0	0,640
July	93,5	50	68,3	92	54	69,1	78	63	69,4	30,20	29,48	29,95	77	45	56,1	2,458
August	80	51	65,5	78	53	66,0	71	63	67,2	30,21	29,55	29,87	70	45	56,7	1,215
September	71	39	57,9	71	39	57,8	67	57	63,0	30,34	29,30	29,56	73	50	61,2	3,780
October	63	35	48,0	62	35	48,7	60	53	56,9	30,31	29,18	29,73	75	50	61,8	3,199
November	56	31	45,7	56	32	46,0	59	51	55,5	30,29	28,74	29,82	73	53	64,6	1,745
December	53	19	37,6	53	20	37,7	57	43	50,2	30,35	29,07	29,76	76	55	66,2	0,733
Whole year			50,5			50,8			58,2			29,87			60,5	18,475

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

Magnetic Needle,

June, 1808.

Variation,   -   24° 10′
Dip   -   -   70° 1′

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCIX.

PART II.

CONTENTS.

PHILOSOPHICAL

TRANSACTIONS.

X. *On Platina and native Palladium from Brasil.* By William Hyde Wollaston, *M. D. Sec. R. S.*

Read March 22, 1809.

ALTHOUGH platina has now been known to mineralogists for more than sixty years, yet it had not been discovered in any other places than Choco and Santa Fé, whence it was originally brought, until about two years since M. VAUQUELIN discovered it in some gray silver ores from Guadalcanal in Estremadura. In analysing these ores, he found some fragments that contained as much as one tenth of their weight of platina, but he did not find it accompanied by any of the new metals that have lately been discovered in the Peruvian ore of platina.

The specimen which I am now about to describe, is derived from a third source, and it is rendered the more interesting by having grains of native palladium mixed with it. This new mineral has lately been received from the gold mines in Brasil, by H. E. Chev. DE SOUZA COUTINHO, ambassador from the court of Portugal, resident in this country, and I am in

hopes that some account of it may be acceptable to the Royal Society, although the analysis must necessarily be very imperfect, from the small quantity to which my experiments have unavoidably been confined.

The general aspect of this specimen is so different from the common ore of platina, that I could form no conjecture of what ingredients it might be found to consist. Its appearance was such indeed, as at first sight to induce a suspicion of its not being in a natural state, for it had very much the spongy form which is given to platina from imperfect attempts to render it malleable by means of arsenic.

One circumstance, however, occasions a presumption that no art has been employed in giving the grains their present appearance; as upon close inspection many small particles of gold are discernible, but there is none of the magnetic iron sand with which the Peruvian ore abounds, nor any of the small hyacinths, which I have formerly noticed as accompanying that mineral.*

It is very well known, that the common ore of platina in general consists of flattened grains, that appear so much worn at their surface, as to be in a considerable degree polished, and the roughness observable in some of the larger grains arises from concave indentations of a reddish brown or black colour. The Brasilian platina, on the contrary, has no polish, and does not appear worn; but most of the grains seem to be small fragments of a spongy substance, and even those which are yet entire and rounded on all sides, present a sort of roughness totally different from that of the former, as their surface consists of small spherical protuberances closely coherent to each

* Phil. Trans. for 1805, p. 318.

other, with the interstices extremely clean, and free from any degree of tarnish.

The first portion that I employed for solution was taken without any selection, and being digested with a small quantity of nitro-muriatic acid, two of the grains were acted on much more rapidly than is usual with platina, and seemed to give a redder colour than that metal alone. These grains were consequently taken out, washed, and reserved for separate examination, and the solution was allowed to proceed till the rest were entirely dissolved. By the addition of muriate of ammonia an abundant precipitate was formed of a bright yellow colour. This precipitate was evidently platina, and its colour satisfied me that the grains had not been brought into their present state from Peruvian platina by means of arsenic; for where arsenic has been employed, I have observed that the iridium contained in that ore is rendered more soluble than before, and thence communicates its red colour to the precipitate.

From the grains thus examined, there appeared not to be any iridium dissolved, nor any black powder containing iridium undissolved.

I next endeavoured, by prussiate of mercury, to ascertain the presence of palladium, but though a precipitate which occurred indicated a certain quantity, it remained doubtful whether it was derived from the grains of platina themselves, or from the two small fragments that had been in part dissolved before they were separated from the rest.

By addition of ammonia to the solution, no iron was precipitated; and when the solution was afterwards allowed slowly to evaporate, I could discern no crystals or colour that I could

ascribe to the presence of rhodium. In short, it seemed that these grains are really native platina nearly pure.

In order to discover whether the grains themselves contained any portion of gold, I selected three of the largest weighing together eight grains and a half; and after a solution and precipitation, as before, by muriate of ammonia, I added a solution of green sulphate of iron, and obtained a precipitate of gold. It was, however, far too small in quantity to be estimated with correctness, but certainly did not exceed the $\frac{1}{200}$ of a grain. This, it is to be observed, is another circumstance in which the present mineral differs from the Peruvian ore of platina, which I believe never contains (in the ore itself) the smallest quantity of gold.

In this experiment also, I tried to detect the existence of palladium in the solution, and by prussiate of mercury again ascertained its presence; but it was in too small quantity for estimating the proportion it bore to the whole mass.

It may deserve to be remarked, that though neither the Peruvian nor Brasilian grains of platina contain any silver, yet the gold which accompanies them is in each instance so much alloyed with silver, that from about thirty small scales of gold picked from Peruvian platina, weighing two grains, I obtained as much as four tenths of a grain of silver, or one fifth part of their weight.

Native Palladium.

The two fragments, that had been separated from the first solution, next claimed my attention, and evidently deserved a careful examination. They were each placed in a drop of nitric acid, and each communicated a deep red colour, which,

by the tests of prussiate of mercury and green sulphate of iron, I was satisfied arose from palladium. The smaller fragment was then divided, and one portion allowed to remain in the acid till it seemed completely dissolved, and the other examined by the blow-pipe. The utmost heat that could be given, appeared to have no effect; but when a small piece of sulphur was applied to it, it fused instantly; by continuance of the heat, it parted with the sulphur, and became completely malleable. In short, it perfectly resembled palladium; and as it retained its brilliancy in cooling, I judged it to be nearly pure.

But as the surfaces which had been acted upon by nitric acid had a degree of blackness, that might be owing to some insoluble impurity, I have since that time dissolved the larger fragment for the sake of discovering the cause of this appearance. Hot nitric acid dissolved by far the greatest part; but there remained a black powder on which a fresh addition of this acid alone had no further effect. But when a drop or two of muriatic acid was added, the whole was very soon dissolved. By the addition of muriate of ammonia, it became evident from the precipitate that the residuum was principally platina. But this precipitate, instead of being yellow, had the deep red colour, which is usually occasioned by the presence of iridium. The platina reduced from this precipitate was also too black for pure platina, and when it was again dissolved, the solution was of a deep red, and the precipitate by muriate of ammonia red, as before; so that although the grains of Brasilian platina appear to be free from iridium, as well as from many other impurities that form part of the Peruvian ore, yet the grains of native palladium that accompany them, afford a trace

of this ingredient, and occasion a presumption that osmium and rhodium may hereafter appear, when we can obtain this mineral in larger quantity.

Since the whole weight of metal employed in the last experiment did not exceed $1\frac{2}{10}$ grain, it is in vain to attempt to estimate the proportion of the ingredients, but if I am near the truth, in considering the quantity of the red precipitate as about one fifth of a grain, of which less than half is platina, those who are best acquainted with the intense colouring power of iridium may endeavour to form a conception of the extremely small quantity that can be present.

As soon as I had ascertained the existence of native palladium, I endeavoured, by examination of its external characters, to distinguish its appearance from that of the surrounding substances, and I found it by no means difficult, although no difference of colour could be discerned. Having remarked that the larger fragment appeared rather fibrous, and that the fibres were in some degree divergent from one extremity, I examined the remainder of the small specimen which had originally been given to me, and by this peculiarity of structure I soon detected a third fragment, which upon trial proved to be the same substance. By favour of the Chev. DE SOUZA I was also permitted, with this view, to examine the specimen which remained in his possession, and had soon the satisfaction of discovering two more fragments of the same mineral, and as I was in no one instance deceived in my choice, by attending to the radiating fibres, I am in hopes that this external character will enable persons to distinguish that metal, in situations where they have not an opportunity of deciding by chemical experiment.

XI. *On a native Arseniate of Lead.* *By the Rev.* William Gregor. *Communicated by* Charles Hatchett, *Esq. F. R. S.*

Read April 13, 1809.

I.

T HAT the oxide of lead and the arsenic acid, might be found in the state of natural combination, is a supposition highly probable, from the strong affinity which subsists between these two substances. But the existence of such a compound has not, as I conceive, hitherto been established by such proofs, as entitle it to be ranked amongst the *decided cases* of mineralogical science. I trust, therefore, that the observations, which I have the honour of submitting to the Society, on a new* ore of lead lately discovered in the county of Cornwall, so justly celebrated as well for the variety as for the richness of its mineral productions, will not be deemed superfluous.

This mineral was raised in the mine called Huel-Unity, a very rich copper mine, in the parish of Gwennap. According to the information with which I have been favoured by Mr. WILLIAM DAVEY, a very intelligent and experienced miner in that district, it was found in a lode south of Huel-Unity principal lode, at the depth of fifty fathoms below the surface,

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* It is new at least to the miners in Cornwall; nor was there, previously to this discovery, any ore resembling it to be found in that splendid collection of minerals, which my valuable friend PHILIP RASHLEIGH, Esq. has so liberally formed, and as liberally employed in the promotion of Science.

which lode underlay about two feet in the fathom south: at the depth abovementioned; this lode fell in, or formed a junction with another small lode or vein to the south, and when the junction took place, this lead ore was found. The veins of it are, in general, from six to ten inches wide, and they diverge on going west. Some particles of this lead ore have been found in the southern part, after the separation of the lodes; but the northern lode does not contain any, until the junction takes place. This ore is intermixed with some native copper, very rich gray copper, and black copper ore, and some is mixed with quartz. The walls of both veins are killas.

II. *Description.*

This mineral is regularly crystallized. The form of its most perfect crystals is an hexaedral prism; they are of different sizes, from one tenth of an inch in diameter, to the size of a hair. The longest which I have seen, do not exceed three tenths of an inch in length: these terminate in a plane, at right angles, with the axis of the prism; but the crystals of a smaller size are frequently drawn out into a very taper acumination, which appears to be a six-sided pyramid. A number of smaller crystals are often closely packed together in bundles, which are bent in different directions, and terminate in a point. The larger crystals, either stand alone, or adhere, on their lateral planes, to the gangue, or are confusedly matted together in a mass.

Some of them are hollow, as if an internal nucleus had been destroyed: and sometimes this internal nucleus overtops the external laminæ. The gangue is a white quartz, which fre-

quently exhibits on its surface the appearance of a partial decomposition.

The red octaedral copper ore, and the copper into which that ore passes, are often intermingled with the crystals of this lead ore and inbedded in them.

The colour of these crystals consists of a variety of tints of yellow. Some are of a beautiful wine yellow resembling the Brazilian topaz: this, in the greater number of specimens, passes into a delicate Isabella-colour: whilst, in other cases, we have the honey-yellow mingled with brown hues of different intensities: so that we meet with crystals resembling dark brown sugar-candy, or common resin.

Some of the crystals are beautifully transparent, whilst others possess this quality in part only, at their extremities, or in inferior degrees throughout their whole lengths.

The external lustre in some specimens, is vitreous; in others, resinous: but in some instances their surface is partially covered by tender and delicate filaments of a silky lustre. These filaments are sometimes found in a separate state loosely adhering to quartz; and they form a variety of this fossil.

The crystals vary as to hardness. The angular fragments of the most transparent are sufficiently hard to scratch glass.

This mineral is easily reduced to powder, which has the appearance of pounded resin; it contracts a yellower tint by long exposure to the air.

The specific gravity of the purest crystals, taken at the temp. 50° FAHRENHEIT, was 6.41.

D d

III.

A fragment of crystal exposed to the flame of the blow-pipe in a gold spoon, melted into a brownish yellow mass, which on cooling did not assume any angular figure. It remained in a state of ignition apparently unaltered; but when a piece of it was exposed to the flame on charcoal, a rapid decomposition took place, arsenical vapours were extricated, and globules of a metal, possessing the common properties of lead, were left behind.

This mineral, in a state of fine powder, is soluble in nitric acid, even without the aid of heat. Care, however, must be taken, that it does not concrete into lumps. The vessel therefore which contains it must be frequently shaken, and the nitrat of lead produced must be, from time to time, dissolved in water, and poured off from the residuum. The process of solution is, however, accelerated by a digesting heat. Some silica remains, which, as the quantity of it is variable according to circumstances, appears not to be an essential ingredient of this fossil.

The nitric solution is colourless; its transparency is not disturbed by nitrat of barytes. Nitrat of silver renders it turbid, and a small quantity of white curdly matter is deposited. Sulphuric acid and the liquid sulphats, produce copious precipitates of a white heavy matter. If the fluid be poured off from this subsided matter, and it be freed from the superfluous sulphuric acid, by the means of nitrat of barytes, it will yield, on the affusion of liquid nitrat of lead, an abundant white precipitate, which urged by the flame of the blow-pipe on a support of charcoal, resolves itself into reduced lead and arsenical vapours.

These preliminary experiments led me to the probable conclusion, that this fossil chiefly consisted of oxide of lead, arsenic acid, and a small quantity of the muriatic acid.

IV. *Analysis.*

A.

1. Fifty grains, carefully selected from crystals of a pale Isabella-colour, were reduced to a fine powder and exposed to a low red heat for about an hour. Their weight was diminished by 0.15 of a grain.

2. The yellowish powder was now transferred to a vessel of pure silver, and mixed with a lixivium containing fifty grains of potash, prepared by the means of alcohol; a quantity, which I had previously ascertained to be sufficient to effect a complete decomposition of this mineral. The ley was gradually evaporated to dryness in a sand-bath. The soluble part was extracted by distilled water, and poured off from a yellowish white matter, which was sufficiently edulcorated (*a*).

3. Liquid nitrat of ammonia was now dropped into the alkaline fluid, as long as it produced any cloudiness: the clear fluid was now decanted from a small quantity of white matter, which had subsided, and rendered acid by nitric acid; ammonia, added to excess, produced a slight turbidness. These precipitates, after sufficient edulcoration, were added to the yellowish white residuum (*a*).

4. The liquid was now rendered slightly acid by nitric acid, and a solution of nitrat* of lead in distilled water was dropped

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* If the colourless liquid *oxynitrat* of lead be dropped into a dilute solution of arsenic acid, or of arseniat of potash acidulated by nitric acid, no immediate precipita-

into it, as long as it separated any precipitate. The clear fluid was poured off, and evaporated nearly to dryness, and a small quantity of white matter, thus obtained, was added to the former precipitate, which dried, and exposed to a low red heat weighed, whilst still warm, 40,8, which, according to the proportion of 33:100, established by Mr. Chenevix, implies 13,46 of arsenic acid.

5. The superfluous lead was now separated from the fluid by sulphat of soda, and filtered off. Ammonia precipitated a minute portion of flaky matter; it weighed, after ignition, 0.2 of a grain; it consisted of silica and oxide of lead, and must be attributed to the nitrat of lead employed.

<div align="center">

B.

</div>

1. The yellowish white residuum (*a*) (A, § 2.) was dissolved without effervescence in nitric acid, except a minute portion of silica, which, after ignition, = 0,28. A white heavy matter was thrown down from this solution, by liquid sulphat of soda. The clear decanted fluid was evaporated to a small volume, and sulphat of soda produced a further separation of white matter; It was sulphat of lead, which after exposure to a low red heat, and weighed, whilst warm, = 47.5, which, upon the supposition that one hundred parts of sulphat of lead contain 69.74 of lead + 3.48 of oxygen, is equivalent to 34.77 of oxide of lead.

2. The fluid, now freed from lead, deposited, on the affu-

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tion of an arseniat of lead is produced; but crystalline grains are, after a time, gradually deposited at the bottom of the vessel. But liquid *nitrat* of lead causes an immediate and abundant precipitate from these same dilute solutions. These two combinations therefore must be different.

sion of ammonia, a greenish matter, which, after ignition, became red, and = 0.033 of a grain. It was oxide of iron.

C.

1. One hundred grains of larger crystals, some of which were hollow, and the surfaces of which were slightly and partially covered with silky filaments, treated in the same way yielded 95.283 of sulphat of lead equivalent to 69.76 of oxide, and 80 of arseniat of lead, which indicates 26,40 of arsenic acid. The oxide of iron, in this case, amounted to only .05 of a grain, and the residuary silica was in too small a quantity to be weighed.

2. I have endeavoured to decompose this fossil by boiling it to dryness in a solution of four times its weight of the purest subcarbonat of potash, and exposing the dry mass, for a very short time, to a low red heat; but I found, that only a part of the arsenic acid had united to the alkali; the larger portion of it was detected in the nitric solution of the residuum; but the relative proportions of the oxide and the acid, were found to correspond almost exactly with the foregoing statement of them.

3. I found also, that carbonat of ammonia precipitated this mineral, *in an unaltered state,* from its solution in nitric acid: as no arsenic acid had united with the precipitant. The solution of the nitrat of ammonia was evaporated to dryness, and exposed to a red heat in a platina crucible; but nothing was left, except a slight trace of oxide of lead. We may infer from hence, the absence of both the fixed alkalies.

4. I found in one specimen only of this fossil any notable difference in the relative proportions of the oxide of lead and

of the acid, to which it is united. It consisted of crystals confusedly matted together in a more compact mass, than this fossil generally assumes. One hundred grains were dissolved in nitric acid; the marine acid was separated by nitrat of silver, and any redundant silver by muriat of ammonia. The lead was separated by sulphuric acid, and the superfluous portion of that acid by nitrat of barytes, and the arsenic acid was combined with the oxide of lead by the addition of nitrat of lead. The muriat of silver $= 9.8$; the sulphat of lead $= 97.6$, and the arseniat of lead $= 72$, equivalent to 1.63 of muriatic acid, $71,46$ of oxide of lead, and $23,88$ of arsenic acid, respectively. The quartz $= 0.35$, and the oxide of iron $.0.2$, nearly,

Another portion taken from the same specimen treated with an alkali, gave very nearly a similar result.

D.

It will now be necessary for me to speak concerning an ingredient of this fossil, which I may have seemed to overlook. I mean the muriatic acid: I have found some difficulty in ascertaining the proportion which it bears to the other constituent parts, and from a cause, which I did not suspect. I considered that the only sure mode of determining this point, was to have recourse to nitrat of silver, which might effect a direct separation of the marine acid from the nitric solution of this fossil. But I found, in many experiments upon given quantities of this mineral, that the results, which I derived from this most valuable chemical test, were variable and uncertain.

At last, I was enabled to trace the error and uncertainty up to two sources. In the first place, I found that the muriat of

silver was more abundant in the cases, where I employed a vessel with a long neck for the solution, and *did not expose it to heat.*

I concluded therefore, that when the process was conducted under different circumstances, the predominating *mass* of nitric acid produced its effect, and volatilized a portion of the muriatic.

Another source of error I found in the following anomalous circumstance, *viz.* a simultaneous precipitation of a portion of arseniat of lead takes place with that of the muriat of silver. Whatever combination this may be, it is a weak one, and may be severed by nitric acid, which dissolves the arseniat and leaves the muriat; or by ammonia, which takes up the muriat, to the exclusion of the arseniat.

The conclusion, to which many experiments have led me, is this, that the muriat of silver produced in the nitric solution of one hundred grains of arseniat of lead by nitrat of silver, amounts to about 9.5.

<p style="text-align:center">E.</p>

In order to prove that the acid, which is combined with the oxide of lead in this mineral, *is the arsenic acid,* and that it is not combined with phosphoric, I decomposed some of its acid, which had been combined with lead in the foregoing experiments, by means of sulphuric acid, and filtered off the sulphat of lead. The fluid which passed through the filter was evaporated nearly to dryness, and it assumed the appearance of crystalline grains. Some of it was exposed to the flame of the blow-pipe in a gold spoon; at first it became like a white dry powder, which melted before an increased heat: placed

on *charcoal* and ignited, it was totally dissipated in arsenical fumes.

Some of it was dissolved in water, and dropped into liquid sulphat of titanium, a white precipitate was produced: combined with soda, it precipitated silver from the nitrat of silver, of a brick colour. It precipitated mercury from its nitrat, of a yellowish colour, which afterwards became reddish. This precipitate exposed to the flame of the blow-pipe on charcoal, exhibited the same phenomena as arseniat of mercury.

I precipitated magnesia from its muriat, and redissolved it by carbonat of ammonia, perfectly saturated with carbonic acid. I divided this liquid into two portions, and dropped into *both* a solution of the combination of the acid of this mineral and soda. No precipitate was produced. I dropped into *one* of the vessels some liquid phosphat of soda, and a separation of saline matter was instantly produced. I soon, however, found, that this mode of distinguishing the phosphoric from the arsenic acid could not be depended upon. For in the other vessel, in which no phosphat of soda had been dropped, in a short time, saline tufts made their appearance, and an abundant deposition of saline matter was formed. I found also, that if the solution had been more concentrated, the precipitation would have immediately taken place.

On making a comparative experiment with arsenic acid, I found that it forms a triple salt with ammonia and magnesia, analogous to the phosphoric salt described by Dr. WOLLASTON. The figure of the arsenical salt, as far as I could determine it from a confused crystallization, is a triedral prism.

We are therefore, I think, authorized from the experiments herein detailed, to conclude, that the fossil which is the subject

of this paper, is an arseniat of lead, and that if we state that the relative proportion of the constituent parts of it are in one hundred, as follows, we shall not be far from the truth:

Oxide of lead	-	69.76
Arsenic acid	-	26,40
Muriatic acid -	-	1.58

The silica and the oxide of iron, which account for a portion of the loss, and the alumina and copper which are sometimes found in an analysis of this fossil, I do not conceive to be essential to it.

The existence of a minute portion of muriatic acid as a constant ingredient of it, is a curious fact: and it is still more curious, when we consider it in connexion with the analogy, that in this particular it maintains with the natural phosphats of lead.

Creed, March 1, 1809.

XII. *An anatomical Account of the Squalus maximus (of* Linnæus), *which in the Structure of its Stomach forms an intermediate Link in the Gradation of Animals between the Whale Tribe and cartilaginous Fishes.* By Everard Home, *Esq. F. R. S.*

Read May 11, 1809.

THE fish from which the following account is taken, was entangled in the herring nets belonging to the fishermen of Hastings, off that coast, and about half-way across the Channel, on the night of the 19th of November, 1808. It was brought ashore at Hastings on the following day, and my late worthy friend, Lieut. Col. BOTHWELL, who was on the spot, purchased it on my account. On the 17th, Mr. CLIFT, the Conservator of the Hunterian Museum, at my desire, went to Hastings, and after making a drawing of the fish, examined its internal structure, and brought to London such parts as were most particularly deserving of notice.

The fish is a male, thirty feet six inches long from the anterior part of the head, to the longest extremity of the tail, and about nine feet from the extreme point of the dorsal fin to the middle line of the belly.

The skin is of a dirty blue, or light slate colour; as rough as a new file in the direction from the tail to the head, but having a sattiny feel in the opposite direction. On the belly the skin is white, thick, and very strong.

The mouth is about five feet from one angle to the other. There are six rows of teeth towards the middle of each side

of the jaw; but in the other parts they are less numerous. The teeth are small, round, conical, very pointed, and bent a little inwards.

The nostrils open on the edge of the upper lip. The eyes are very small, and the pupils perfectly round.

Half-way between the eye and the gills, on each side, is the orifice of a canal, which leads into the mouth.

The gills are five in number on each side.

The pectoral fins are situated a little behind the posterior gills.

The dorsal fin is situated nearly opposite to the middle space, between the pectoral and anal fins. The posterior dorsal fin is small, and situated half-way between the anal fins, and the setting on of the tail. The two anal fins are attached on their upper edge for about half their extent each to the lower side of a long projecting body peculiar to the male. All the fins have a thick round edge anteriorly, and become gradually thinner towards the posterior part, which is partially serrated.

The projecting bodies on the sides of the anus I shall call holders, as they are employed in grasping the female in the time of copulation. They are rounded on their lower surface: the skin covering them is uncommonly thin and smooth: on their upper surface, it has a gloss like that of silk, and there is a deep sulcus, in which is contained a strong, flat, sharp, bony process, five inches long, which moves on a joint, and the bone projects an inch and a half beyond the skin, like a spur.*

There is a deep sulcus at the setting on of the tail, as if a

* I shall give a particular description of these holders in a future paper on the generation of the dog-fish.

rope had been tied tight round that part, and on each side of the fish, there is a scabrous ridge extending from this sulcus as far forwards as the posterior dorsal fin. The tail may be said to begin from this deep transverse sulcus; it is vertical; the upper portion is longest and narrowest. The thin edge of the tail has a jagged appearance, as if formerly wounded by the bites of small fishes.

The proportions of these parts to each other will be seen in the annexed figure.

In this fish no part of the skeleton can be said to be perfectly formed bone, although the skull, which defends the brain, the upper and under jaws, and the vertebræ, contain bony matter; the vertebræ however in much the smallest proportion.

The skull has no cartilaginous attachment to the upper jaw, so that it is readily separated from it, while the jaws themselves are strongly connected by means of the joint between them. The skull, when the jaws are removed, is very small, being adapted to the size of the brain, which in this fish bears a very small proportion to that of the whale tribe.

The vertebræ next the head are rather smaller than those a little further on. The largest are seven inches in diameter, the cavities between them containing about three pints of fluid. A particular account of the intervertebral joint in this fish has been already laid before the Society.

On the posterior part of the vertebræ, there is a sulcus, in which a strong elastic ligament passes from the head almost to the tail. The canal for the spinal marrow is very small; and on the anterior part of the vertebræ, below the anus, there is a canal, in which is contained the aorta.

Each gill is supported on two cartilages, one long and flat connected with the spine, the other short; there is a joint between them which admits of some degree of motion. There are cartilages which answer the same purpose, as the sternum and scapulæ of quadrupeds. Those resembling the scapulæ are connected to the spine, and to them the cartilages of the pectoral fins are articulated.

The broad part of the pectoral fin is formed of a number of cartilaginous finger-like processes. The terminations of which are inclosed between two sets of ligamentous fibres, that form the thin margin of the fin.

The other fins and tail are formed in the same manner, and are also connected with the spine.

There is an imperfectly formed pelvis, connected to the spine by strong ligaments and muscles. The cartilages of the holders are united to the pelvis; they correspond in number and general appearance to a femur, tibia, and three toes. This part has been frequently mistaken by superficial observers for the leg and foot, to which it bears a very general resemblance.

The heart is not much larger than that of a bullock: the auricle is extremely thin in its coats. The valves at the origin of the pulmonary artery, are three in number; besides which there are three sets of valves in the course of the artery, at a short distance from each other. Each set is composed of three valves. They are weaker than those at the origin of the artery, and they are attached by tendinous chords to the inside of that vessel.

The tongue is flat, and can hardly be said to have any part of it loose and pendulous.

The œsophagus is about a foot in length: its internal surface is milk white and polished, having a number of conical papillæ projecting into the canal. These become gradually longer towards the stomach; they all point downwards, and the lower ones have a fringed appearance, which is shewn in the figure.

The stomach, from its weight, could not be removed till emptied of its contents, which consisted of several pails full of pebbles, a quantity of mucus, and a small portion of a substance, which proves to be the spawn of a univalve. The appearance of the internal structure of the stomach, and the dimensions of its different parts are shewn in the drawing. Besides the cardiac and pyloric portions, as in other sharks, there is a globular cavity, with which the pyloric portion communicates by a very small orifice, and there is another orifice nearly of the same size, between this cavity and the intestine. The upper part of the duodenum is smooth, and the gall ducts open into it by a long nipple-like projection, and just below this the spiral valve has its origin, as in other sharks.

The valvular intestine, from the closeness of the turns of the valvular part, is so firm and compact, that when placed on its end, it stood upright like a cask. It is about four feet long, and ten inches in diameter, terminating at its lower end in the appearance of a rose, which is so remarkable, that it is represented in the annexed figure. Below this the rectum begins, which in this instance was two feet long. Behind the rectum, and loosely attached to the spine, is an oval bag, the coats of which are very strong: its internal membrane is reticulated, forming very deep folds, and there is a long narrow duct leading from its cavity into the rectum. This bladder

contains a dark-coloured glary fluid, and this is common to the shark tribe, but the use of such a secretion is not at present known.

The spleen in all respects resembles that of the blue shark formerly described.

The pancreas is situated in the angle between the pylorus and duodenum. It was so much broken, that its shape could not be ascertained; but its substance is composed of a soft whitish mass, intermixed with roundish bodies of a firmer texture.

The liver consists of two lobes nearly equal in size. They occupy the anterior part of the belly, from below the grills to the rectum. It yielded about three hogsheads of oil. No gall-bladder was discovered; and as a chord (like a navel string) consisting of twelve hepatic ducts passed from the liver to the duodenum, there is reason to believe that this fish has no gall-bladder. There is none in the piked whale. On cutting into the liver, the blood-vessels were found to be so large, that they readily admitted a man's arm, and on pulling them the substance of the liver was readily torn in the direction of the smaller branches, which went off at right angles to the central trunk, as regularly as those of the gills.

The kidnies are long narrow bodies, lying on each side of the spine, and extending along the whole length of the abdomen. The ureters run along their inner edge, and terminate in an oval cavity just within the verge of the anus, which has an imperfect septum separating it into two parts, the ureters opening on the opposite sides of this septum, this cavity must therefore be considered as the urinary bladder.

The testicles are situated immediately behind the origin of

the two lobes of the liver. They are oval, flattened, pulpy bodies. The epididymis nearly surrounds the testicle, and then forms the *vas deferens*, which makes many close turns upon itself, and passes downwards, adhering to the anterior surface of the kidney, in which it is in some measure embedded: the lowest part of the vas deferens, for three feet in length, becomes very large, and has no convolutions. The vasa deferentia contained a substance like thin starch, broken down into small rounded portions, mixed with a thinner fluid. These ducts were so large, as readily to allow a man's arm to be introduced up to the shoulder. Each vas deferens terminates by a small contracted orifice in the urinary bladder, one on each side, so that this bladder is both a reservoir for the urine and semen. The bladder in the male opens externally by an infundibular process, which constitutes the penis.

On each side of the anus, within its verge, near the root of the penis, is an oblique aperture, communicating freely with the cavity of the abdomen.

From the account which has been given, the Squalus maximus appears in many respects to be similar in its structure to the shark, but it differs essentially from it in the form of the stomach, and in that respect forms an intermediate link between the shark and whale. It probably lives on nearly the same kinds of food as the whale.

The sharks form a tribe of such extent, that from what we already know of their internal structure, they may be subdivided into many genera, making with the rays and scates, so many links between the whales and fishes, properly so called. The stomach and organs of generation are the parts in which the structures are most essentially different. In the

dog-fish, the stomach in its form is neither like that of the Squalus maximus, nor blue shark. I have given a drawing of it, as it appears to form a link between the two. The structure of the ovum, and the mode of hatching it is very different in the dog-fish from that of many other sharks ; as I have had opportunities of investigating the mode of generation in that species, I shall make it the subject of a future communication.

I cannot close the present paper without mentioning, that nearly about the same period, two other Squali of large dimensions were thrown upon our coast. The probable cause of this event, is the season being uncommonly boisterous and tempestuous. On the 3d of January, 1809, a fish was thrown ashore at Penrhyn, in Cornwall. On hearing of it from a person on the spot, I sent down a drawing of the subject of this paper to compare with it, and the fish proves to be of the same species, and a male, measuring thirty-one feet in length.

The other was thrown ashore on the 7th of October, 1808, at Rothiesholm, an estate of GILBERT MEASON, Esq. in Stronsay, one of the Orkney isles. It had been seen lying on some sunken rocks, eleven days before, was in a half putrid state, and the sea fowl were in great numbers feeding upon it. Those who saw it, reported that the skin was rough in one direction, and smooth like satin in the other. At the time of its being examined, the skin and a great many other parts of the fish were wanting.

Mr. MEASON, with a zeal for science which does him infinite credit, upon hearing the strange accounts which were given of this sea monster, got his brother, MALCOLM LAING, Esq.

and Dr. GRANT, an eminent physician (both justices of the peace), to take depositions on the spot, from those persons who had seen the fish, that its real appearance might be ascertained. This examination, however, did not take place till six weeks after the fish was thrown ashore.

These depositions were sent to Sir JOSEPH BANKS, who put them into my hands.* I also received, a short time after, from my friend Mr. LAING, in consequence of a request I made for that purpose, that part of the skull, which contained the brain, the upper jaw having been separated from it, a considerable number of the vertebræ of the back united together by their natural attachments, a portion of one of the pectoral fins, with the cartilages that unite it to the spine, and a long and short cartilage forming the support of one of the gills. On comparing these different parts, with those of the Squalus maximus, they were found to agree, not only in their form, but also in their dimensions. This led to the opinion of the fish being a Squalus, a very different one from what was formed by those who saw it in the mutilated state in which it was thrown ashore, and who called it a *sea snake*. In the different depositions, several parts are accurately described, such as the valvular intestine, which was taken for the stomach, and the bristles of the mane, which are described as ligamentous fibres: one of them is in my possession, and is of the same kind with the fibres forming the margin of the fins of the Squalus maximus. The drawing that was made from memory, and which I have annexed, will enable me in a few words to point out how far, in some things, those who saw the fish adhered to

truth, and in others allowed their imaginations to supply deficiencies, for one of them declared, with confidence, that the drawing was so exact a representation of what he had seen, " that he fancied he saw the beast lying before him, at a dis- " tance, on the beach."

The drawing is correct in the representation of the head and anterior part of the fish, from which the skin, the upper and lower jaw, the gills, and gullet, had been separated by putrefaction ; and when we consider that the liver and the other viscera were all destroyed, except the valvular intestine, which was taken away by the observers, the size of the body that remained would be nearly in proportion with the drawing. The legs are tolerably exact representations of the holders in the male Squalus maximus, described in a former part of this paper, and therefore are not imaginary, only that four have been added which did not exist. This is satisfactorily determined by the pectoral fin, which is preserved, having no resemblance to them. The mane, they said, was composed of ligamentous fibres, one of which was sent to London ; this corresponds, in its appearance, with the fibres that form the termination of the fins and tail of the Squalus maximus, such an appearance therefore was seen, but could only be met with in the place of the two dorsal fins, instead of being continued along the back, as in the drawing. The contortions towards the tail are such, as the intervertebral joints could not admit of, they are therefore imaginary.

It is said, two different persons measured the fish ; one by fathoms, the other by a foot rule, and that it was fifty-five feet long. Their accuracy is at least doubtful, as the parts that are preserved correspond with those of a fish about

thirty feet long, and it is rendered still more so, as the person who gives the length in fathoms, says, he saw at that time the six legs, the two foremost being larger than the hinder ones, and the lower joint more rounded from the body to the toes. The pectoral fin, which is preserved, proves this declaration to be incorrect: the person who measured the fish with a foot rule, declares the length, from the hole in the head to the beginning of the mane, to be exactly fifteen feet, which is probably correct, since a Squalus of about thirty-six feet long would measure, from the fore part of the skull to the dorsal fin, about fifteen feet; but the other measurement must be questionable.

It is deserving of remark, that there is no one structure represented in this drawing, which was not actually seen. The skeleton of the holders corresponds with the legs in the drawing, the margin of the dorsal fin in a putrid state with the mane; so that the only errors are in the contortions towards the tail, the length of the fish, and the number of the holders, which were mistaken for legs :* and when we recollect that the drawing was made from memory six weeks after the fish had been seen by those, who describe it, during which interval it had been their principal subject of conversation, we may conclude that so extraordinary an object, as the mutilated fish must appear when believed to be a perfect one, would, in their different discourses, have every part exagge-

* This mistake of the holders of the male shark for legs, has been frequently made. ... a drawing in Sir JOSEPH BANKS's library, sent from Ireland, in which the ... walking like a duck, with broad webbed feet. The skin of a male ... exhibited in London some years ago ... ... its legs, on which it occasionally walked.

rated, and it is only remarkable that the depositions kept so close to the truth as they have done.

It is of importance to science, that it should be ascertained, that this fish is not a new animal unlike any of the ordinary productions of nature, and we are indebted to the zeal and liberality of Mr. MEASON and Mr. LAING, who have collected a sufficient body of evidence to enable me to determine that point, and prove it to be a Squalus, and the orifice behind the eye, which communicates with the mouth met with in the skull, renders it very probable that it is a Squalus maximus.

This opinion is further confirmed by the Squalus maximus, known by the name of the basking shark, being frequently seen upon the coast of Scotland.

That a fish so common in the northern seas, containing a large quantity of oil, should have been so rarely caught, and indeed that it should not, as well as the whale, become an object of the Greenland fishery, appears, on the first view, not easily accounted for; but Sir JOSEPH BANKS has thrown out a suggestion which satisfactorily explains it. The whale, when struck, descends towards the bottom of the sea, but is soon obliged to rise to the surface to breathe, which enables the fishermen to follow, and prevent the breaking of their line; but the Squalus maximus, as it breathes water, has no occasion to return to the surface, and will always carry off the line.

EXPLANATION OF THE PLATES.

PLATE VI.

Fig. 1. An engraving of the Squalus maximus caught at Hastings in 1808.

a. A projecting body peculiar to the male. There is a pair of these, by means of which the female is held in the act of copulation.

Fig. 2. An exact copy of the drawing sent up to Sir Joseph Banks from the Orkneys, of a Squalus in a mutilated state, without the skin or viscera, thrown ashore upon that coast, which being mistaken for a perfect animal, unlike any thing at present known, it was supposed to be a sea snake.

a. The skull, from which the upper and lower jaw had been separated by putrefaction.

b. The orbit.

c c. The spine of the fish surrounded by muscles, the gills and gullet having been separated by putrefaction.

d d. The dorsal fin, which in a half putrid state puts on a shaggy appearance, the ligamentous fibres of which the broad part is composed being separated from one another; this was mistaken for a mane, and, from want of accurate observation, was continued on to the tail, although it could only be seen in the situation of the two dorsal fins.

e e, e e, e e. The holders of the male, which are represented with tolerable accuracy as they appear in a half putrid state, but two only are met with in nature in the situation in which they are seen in Fig. 1; there they have a different appearance, the internal parts being concealed by the common integuments.

f f f. Contortions which the structure of the intervertebral substance of the fish rendered it impossible for the spine to assume, and therefore could not have been seen. These contortions, if represented, render it highly probable that the account of Pontoppidan's sea snake had been read by the

spectators of this fish, in the interval of time between their seeing it and their depositions being taken.

PLATE VII.

The stomach of the Squalus maximus laid open to shew the appearance of its internal structure.

a. The internal membrane of the œsophagus exposed to view.

b. The termination of the œsophagus in the stomach by a loose kind of fringe.

c c. The cardiac portion of the stomach, the upper part of which is slightly honey-combed; the lower having more of a rugous, or plicated appearance.

d d. The pyloric portion, the coats of which are very strong, and the internal rugæ very thick. The opening at the pylorus is very small, as in the whale tribe.

e e. The external surface of the valvular intestine.

f. The ducts of the liver passing down in the form of a broad band to the duodenum.

g g g. The spleen.

PLATE VIII.

Fig. 1. Shews the internal surface of a small cavity interposed between the pyloric portion of the stomach and the intestine. •

a a. The external surface of the pyloric portion of the stomach.

b. The cavity, with which it communicates, laid open, exposing the small opening between them, and another opening leading to the intestine.

c. The cavity of the intestine before the valvular structure begins.

d. The orifice leading into it from the cavity next the stomach.

e. The opening of the ducts from the liver.

f. The valvular portion of the intestine.

g. A portion of the spleen.

h h. The ducts passing from the liver.

Fig. 2. The termination of the valvular portion of the intestine in a rose-like form; by means of this structure the aliment is prevented from too readily making its escape.

PLATE IX.

The stomach of the common dog-fish laid open, to shew the difference of its internal structure from that of the Squalus maximus, and also the difference between it and that of the common shark.

a. The internal membrane of the œsophagus.

b. The termination of the œsophagus.

c c. The cardiac portion of the stomach, the superior part of which is plicated, and the inferior slightly honey-combed, the very reverse of what is met with in the Squalus maximus.

d. The pyloric portion.

e. The pylorus,

f. A small space between the pylorus and the intestine, bearing a faint resemblance to the cavity in the Squalus maximus, but so slight, as only to be detected when the fish is in a very recent state.

g. The beginning of the intestine.

h. The valvular portion.

I

Fig. 2.
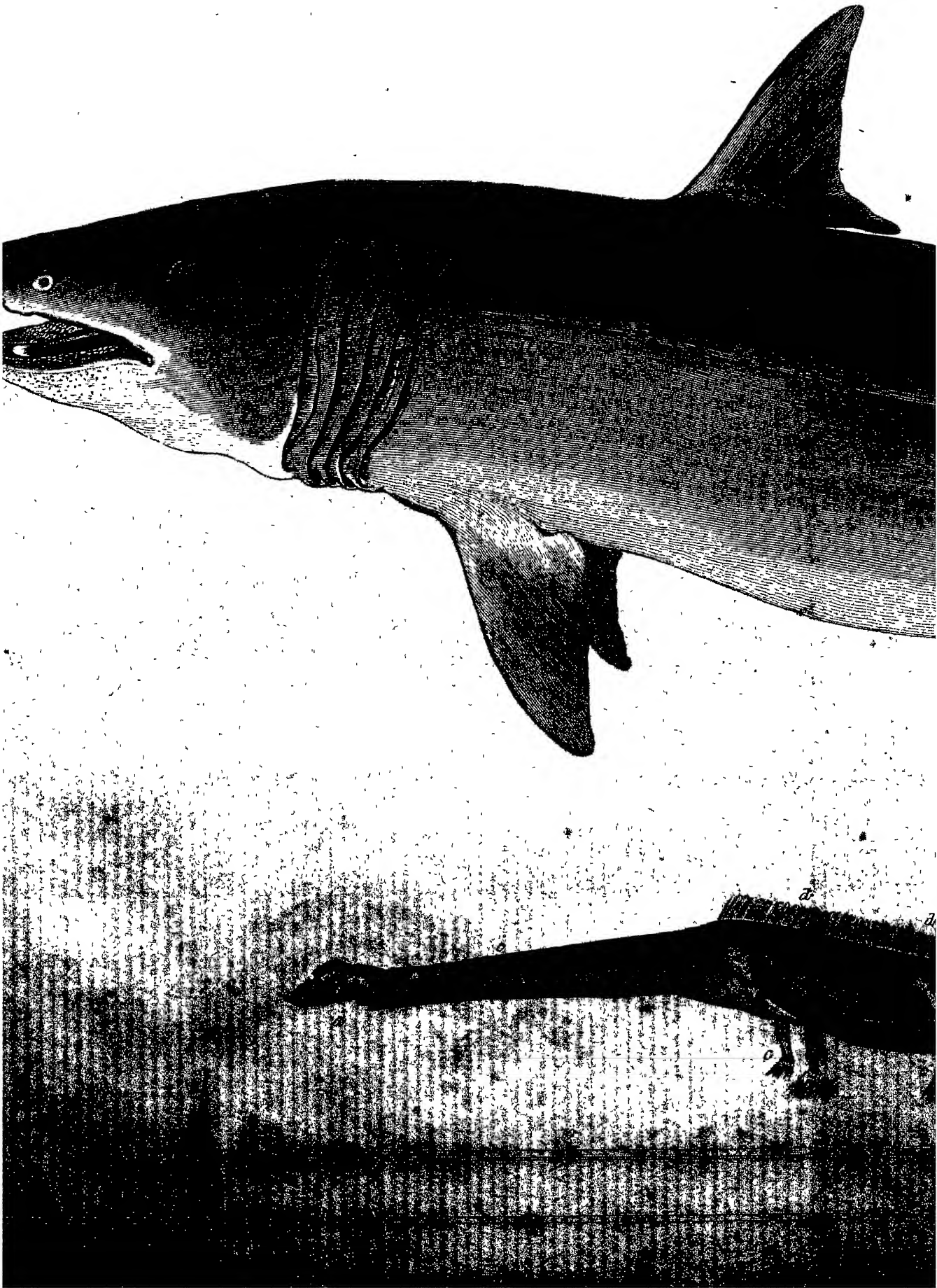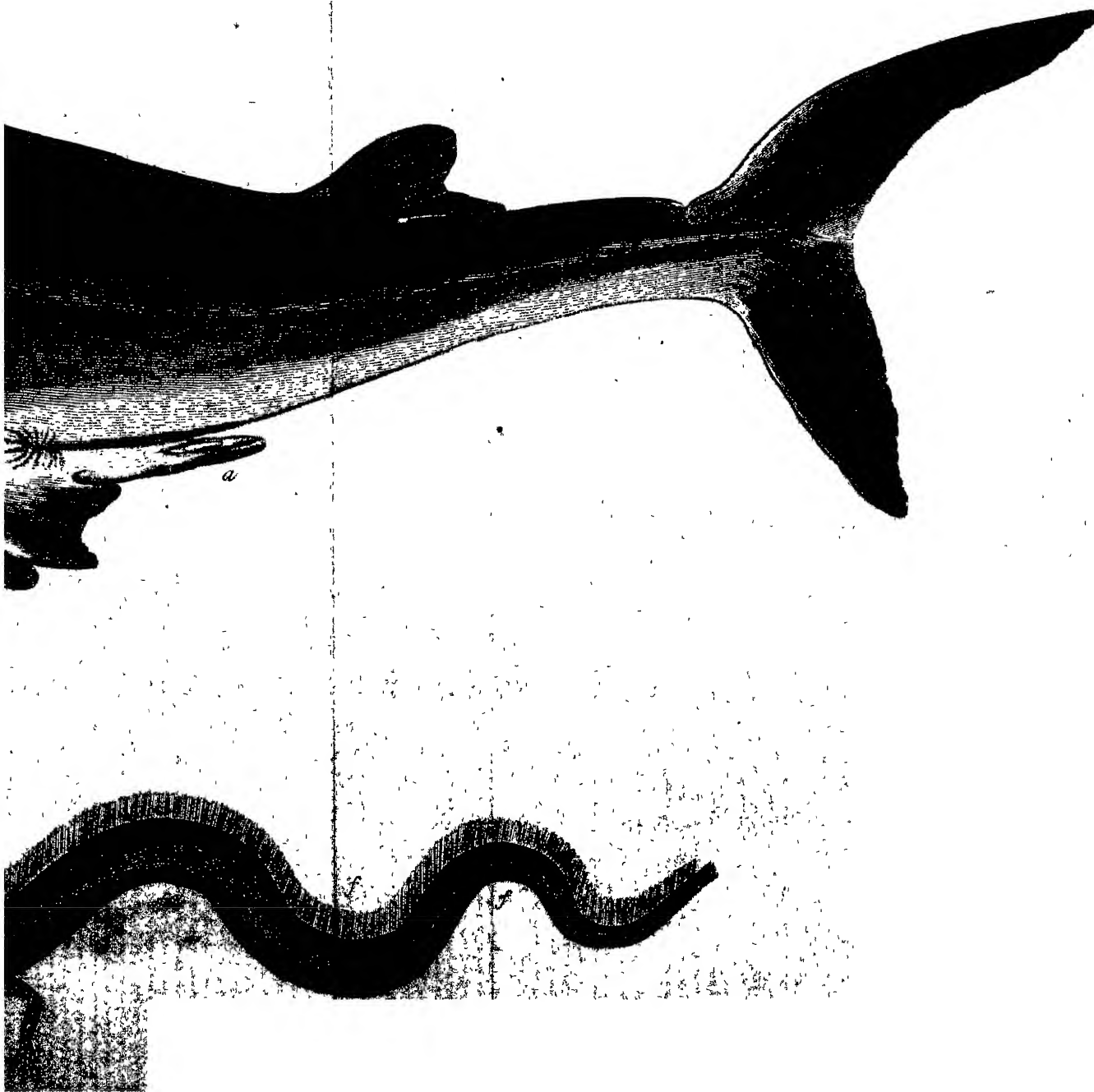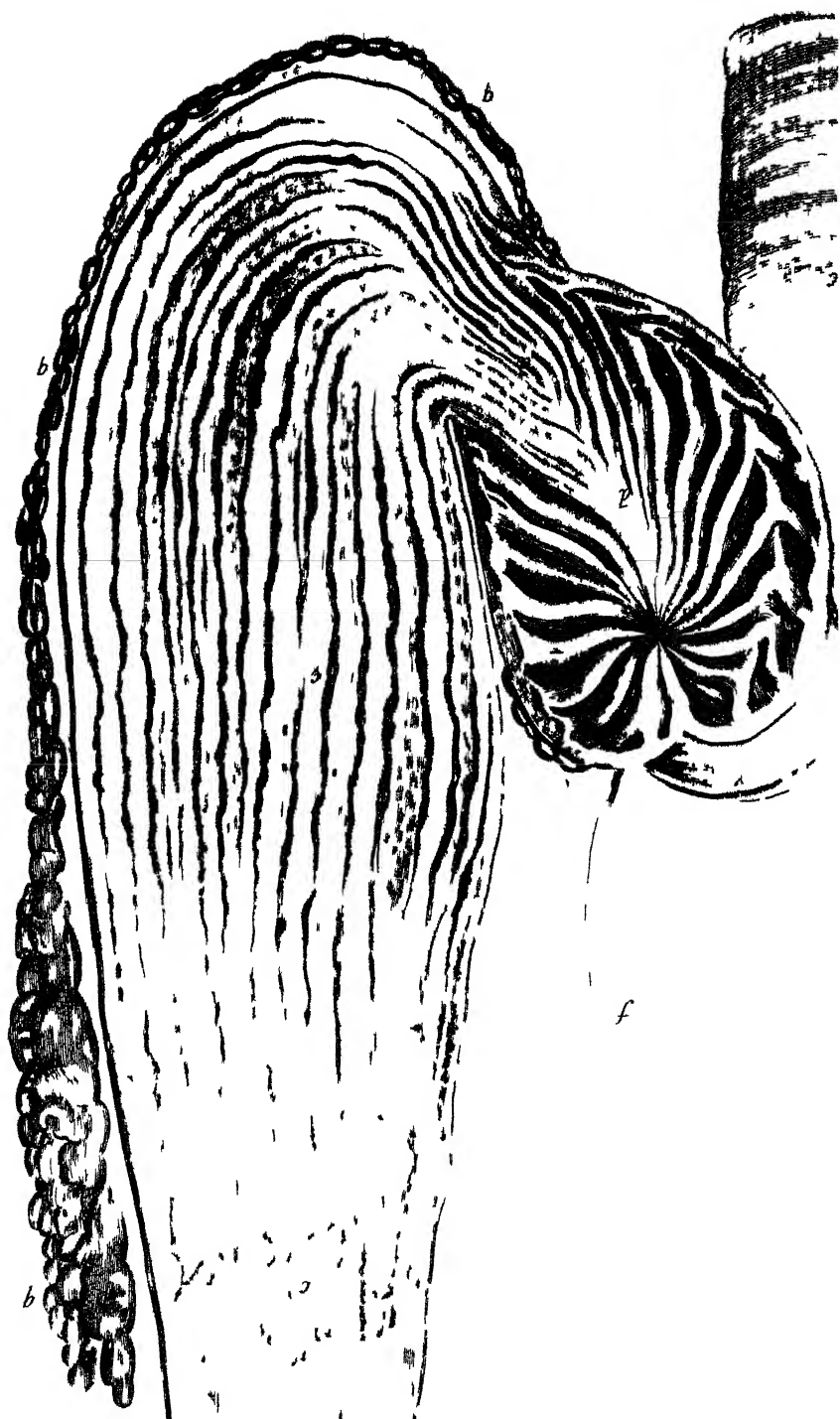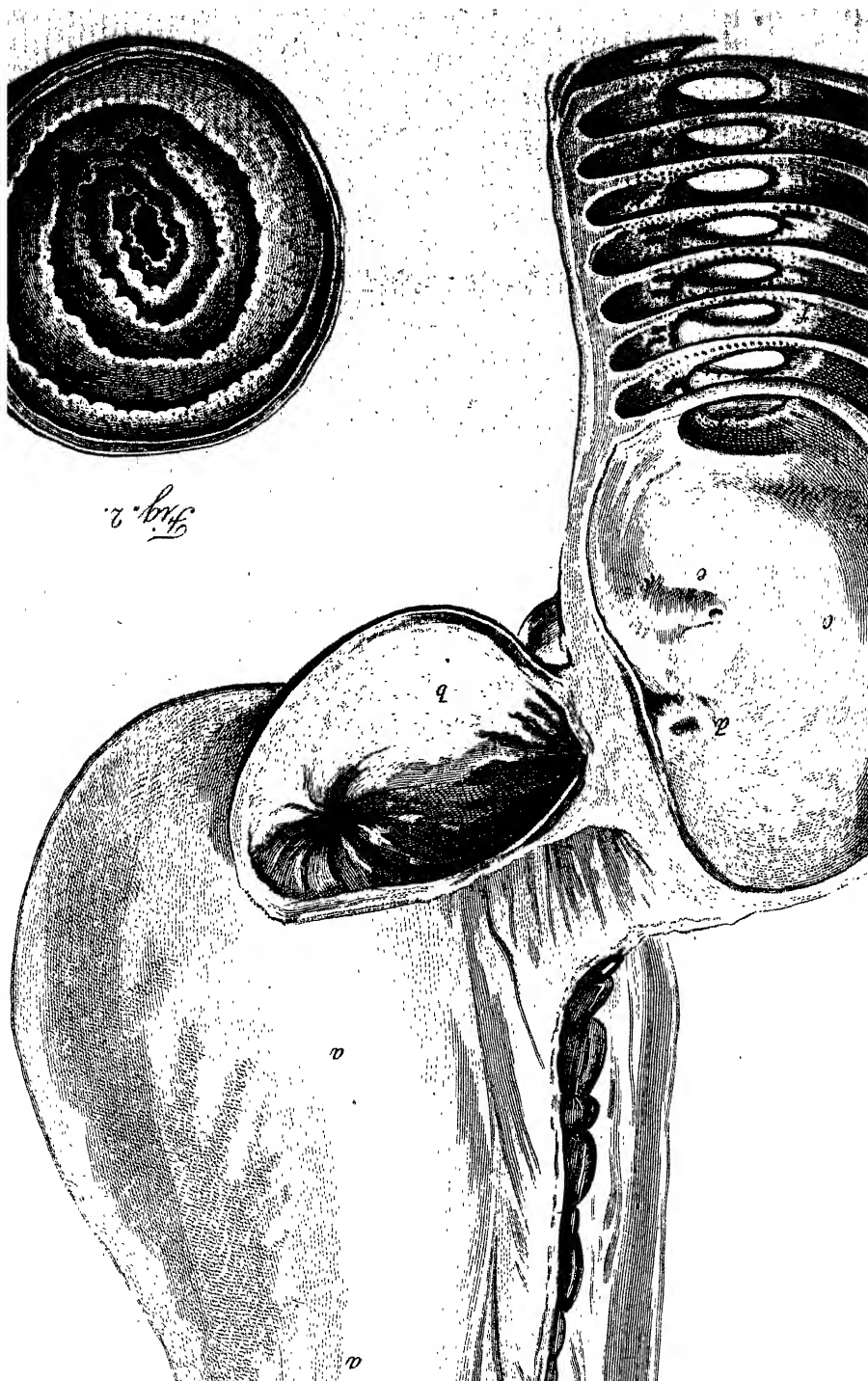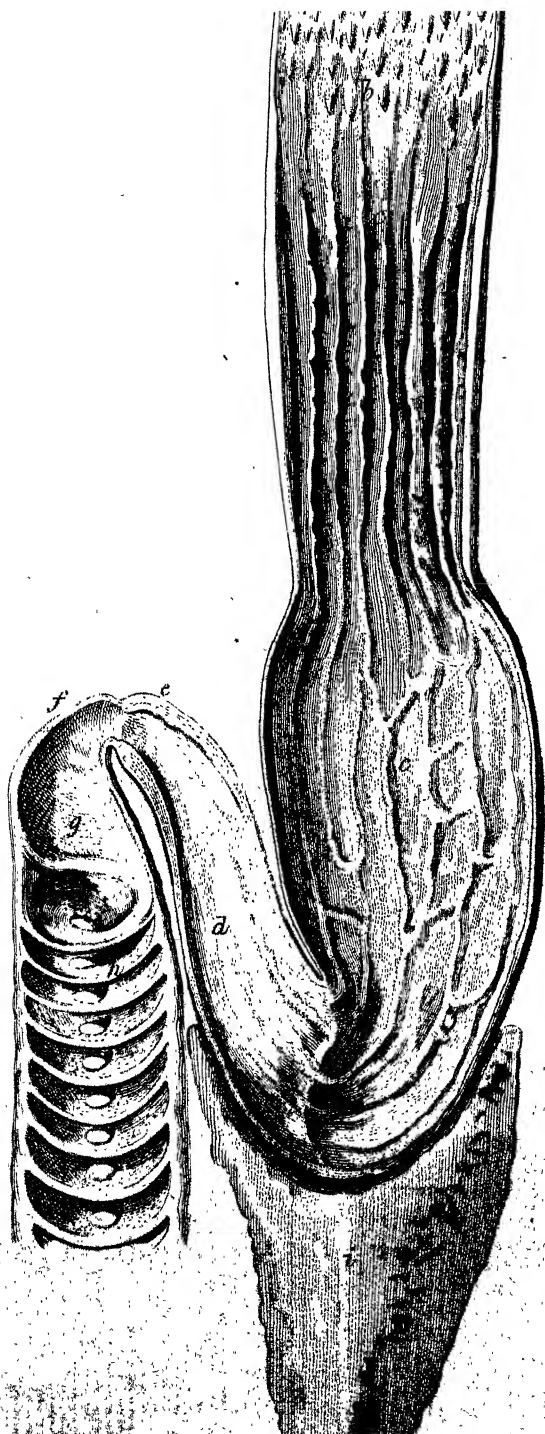
XIII. *On an Improvement in the Manner of dividing astronomical Instruments.* By Henry Cavendish, *Esq. F. R. S.*

Read May 18, 1809.

THE great inconvenience and difficulty in the common method of dividing, arises from the danger of bruising the divisions by putting the point of the compass into them, and from the difficulty of placing that point mid-way, between two scratches very near together, without its slipping towards one of them, and it is this imperfection in the common process, which appears to have deterred Mr. TROUGHTON from using it, and thereby gave rise to the ingenious method of dividing described in the preceding part of this volume. This induced me to consider, whether the abovementioned inconvenience might not be removed, by using a beam compass with only one point, and a microscope instead of the other; and I find, that in the following manner of proceeding, we have no need of ever setting the point of the compass into a division, and consequently that the great objection to the old method of dividing is entirely removed.

In this method, it is necessary to have a convenient support for the beam compass: and the following seems to me to be as convenient as any. Let C C C (Fig. 1.) be the circle to be divided, B B B a frame resting steadily on its face, and made to slide round on it with an adjusting motion to bring it to any required point: $d\delta$ is the beam compass, having a point near

G g

δ, and a microscope m made to slide from one end to the other. This beam compass is supported at d, in such manner as to turn round on this point as a centre, without shake or tottering; and at the end δ it rests on another support, which can readily be lowered, so as either to let the point rest on the circle, or to prevent its touching it. It must be observed, however, that as the distance of d from the center of the circle must be varied, according to the magnitude of the arch to be divided, the piece on which d is supported had best be made to slide nearer to, or further from, the center; but the frame must be made to bear constantly against the edge of the circle to be divided, so that the distance of d from the center of this circle, shall not alter by sliding the frame.

This being premised, we will first consider the manner of dividing by continued bisection. Let F and f be two points on this limb which are to be bisected in φ. Take the distance of the microscope from the point nearly equal to the chord of $f\varphi$, and place d so that the point and the axis of the microscope shall both be in the circle in which the divisions are to be cut. Then slide the frame B B B till the wire of the microscope bisects the point F; and having lowered the support at δ, make a faint scratch with the point.

Having done this, turn the beam compass round on the center d till the point comes to D, where it must rest on a support similar to that at δ; and having slid the frame till the wire of the microscope bisects the point f, make another faint scratch with the point, which if the distance of the microscope from the point has been well taken, will be very near the former scratch; and the point mid-way between them will be the accurate bisection of the arch Ff; but it is unnecessary,

and better not to attempt to place a point between these two scratches.

Having by these means determined the bisection at φ, we must bisect the arches $F\varphi$ and $f\varphi$ in just the same manner as before, except that the wire of the microscope must be made to bisect the interval between the two faint scratches, instead of bisecting a point.

It must be observed, that when the arch to be bisected is small, it will be necessary to use a bent point, as otherwise it could not be brought near enough to the axis of the micro-scope; and then part of the rays, which form the image of the object seen by the microscope, will be intercepted by the point; but I believe, that by proper management this may be done without either making the point too weak, or making the image indistinct; but if this cannot be done, we may have recourse to Mr. Troughton's expedient of bisecting an odd number of contiguous divisions.

It must be observed too, that in the bisections of all the arches of the same magnitude, the position of the point d on the frame remains unaltered; but its position must be altered every time the magnitude of the arch is altered.

It is scarcely necessary to say, that the bisections thus made are not intended as the real divisions, but only as marks from which they are to be cut. In order to make the real divisions, the microscope must be placed near the point, and the support d must be placed so that $d\delta$ shall be a tangent to the circle at δ. The wire of the microscope must then be made to bisect one of these marks, and a point or division cut with the point, and the process continued till the divisions are all made.

It is plain that in this way, without some further precaution,

we must depend on the microscope not altering its position in respect of the point during the operation; for which reason I should prefer placing the axis of the microscope at exactly the same distance from the center of motion d, as the point; but removed from it sideways, by nearly the semi-diameter of the object glass; so that having made the division, we may move the beam compass till the division comes within the field of the microscope, and then see whether it is bisected by the wire, and consequently see whether the microscope has altered its place,

In the operation of bisection, as above described, it may be observed, that if the two scratches are placed so near together, that in making the second the point of the compass runs into the burr raised by the first, there seems to be some danger that the point may be a little deflected from its true course; though in BIRD's account of his method, I do not find that he apprehends any inconvenience from it. One way of obviating this inconvenience, if it does exist, would be to set the beam compass not so exactly to the true length, as that one scratch should run into the burr of the other; but as this would make it more difficult to judge of the true point of bisection, perhaps it might be better to make one scratch extend from the circle towards the center, and the other from it.

It is clear, that the entire arc of a circle cannot be divided to degrees, without trisection and quinquesection; and I do not know whether our artists have recourse to this operation, or whether they avoid it by some contrivance similar to BIRD's, that of laying down an arch capable of continued bisection; but if the method of quinquesection is preferred, it may be performed by either of the three following methods:

First Method.

Let $a \alpha$ (Fig. 2) be the arch to be quinquesected. Open the beam compass to the chord of one fifth of this arch; bring the microscope to a, and with the point make the scratch f; then bring the microscope to f, and draw the scratch e; and in the same manner make the scratches d and b. Then turn the beam compass half round, and having brought the microscope to α, make the scratch β; and proceeding as before, make the scratches δ, ϵ and ϕ. Then the true position of the first quinquesection will be between b and β, distant from β by one fifth of $b\beta$; and the second will be distant from δ by two fifths of $d\delta$, and so on.

Then, in subdividing these arches, and striking the true divisions, the wire of the microscope, instead of bisecting the interval between the two scratches, must be brought four times nearer to β than to b. But in order to avoid the confusion which would otherwise proceed from this, it will be necessary to place marks on the limb opposite to all those divisions, in which the interval of the scratches is not to be bisected, shewing in what proportion they are to be divided; and these marks should be placed so as to be visible through the microscope, at the same time as the scratches. Perhaps, the best way of forming these marks, would be to make dots with the point of the beam compass contiguous to that scratch which the wire is to be nearest to, which may be done at the time the scratch is drawn.

Perhaps an experienced eye might be able to place the wire in the proper manner, between the two scratches, without further assistance; but the most accurate way would be to

have a moveable wire with a micrometer, in the focus of the microscope, as well as a fixed one; and then having brought the fixed wire to b, bring the moveable one to β, and observe the distance of the two wires by the micrometer; then reduce the distance of the two wires to one fifth part of this, and move the frame till the moveable wire comes to β, and then the fixed wire will be in the proper position, that is four times nearer to β than to b.

It will be a great convenience, that the moveable wire should be made in such manner, as to be readily distinguished from the fixed, without the trouble of moving it.

In this manner of proceeding, I think a careful operator can hardly make any mistake: for if he makes any considerable error in the distance of the moveable wire from the fixed, it will be detected by the fixed wire not appearing in the right position, in respect of the two scratches; and as the mark is seen through the microscope, at the same time as the scratches, there is no danger of his mistaking which scratch it is to be nearest to, or at what distance it is to be placed from it.

To judge of the comparative accuracy of this method with that of bisection, it must be considered that the arches $\alpha\beta$, $\beta\delta$, &c. though made with the same opening of the compass, will not be exactly alike, owing partly to irregularities in the brass, and partly to other causes. Let us suppose, therefore, that in dividing the arch $a\alpha$ into five parts, the beam compass is opened to the exact length, but that from the abovementioned irregularities the arches $\alpha\beta$, $\beta\delta$, $\delta\varepsilon$, and $\varepsilon\varphi$ are all too long by the small quantity ε, and that the arches af, fe, ed, and db are all too short by the same quantity, which is the supposition the most unfavourable of any to the exactness of

the operation ; then the error in the position of $\beta = \epsilon$, and the point b errs 4ϵ in the same direction, and therefore the point assumed as the true point of quinquesection, will be at the distance of $\frac{3\epsilon}{5}$ from β, and the error in the position of this point $= \epsilon \times 1\frac{3}{5}$.

By the same way of reasoning, the error in the position of the point taken between d and $\delta = \epsilon \times 2\frac{2}{5}$.

In trisecting the error of each point $= \epsilon \times 1\frac{1}{3}$; and in bisecting, the error $= \epsilon$; and in quadrisecting, the error of the middle point $= 2 \epsilon$.

It appears therefore that in trisecting, the greatest error we are liable to does not exceed that of bisection in a greater proportion than that of 4 to 3; but in quinquesecting the error of the two middle points is $2\frac{2}{5}$ times greater than in bisecting. It must be considered, however, that in the method of continued bisection, the two opposite points must be found by quadrisection ; and the error of quinquesection exceeds that of quadrisection in no greater proportion than that of six to five; so that we may fairly say, that if we begin with quinquesection, this method of dividing is not greatly inferior, in point of accuracy, to that by continued bisection.

Second Method.

This differs from the foregoing, in placing dots or scratches in the true points of quinquesection and trisection, before we begin to subdivide. For this purpose, we must have a microscope placed as in page 224, first par. at the same distance from the center of motion as the point is ; and this microscope must be furnished with a moveable wire and micrometer, as in page

226; and then having first made the fixed wire of this micro-
scope correspond exactly with the point, we must draw the
scratches *b* and *β*, *d* and *δ*, &c. as before, and bring the
fixed wire to the true point of quinquesection between *b* and *β*,
n the manner directed in page 226, and with the point strike
the scratch or dot; •and if we please, we may, for further
security, as soon as this is done, examine, by means of the
moveable wire, whether this intermediate scratch or dot is
well placed.

The advantage of this method is, that when this is done,
we may subdivide and cut the true divisions, by making the
wire of the microscope bisect the intermediate scratches, in-
stead of being obliged to use the more troublesome operation
of placing it in the proper proportion of distance between the
two extremes.

This method certainly requires less attention than the for-
mer, and on the whole seems to be attended with consider-
ably less trouble; but it is not quite so exact, as we are liable
to the double error of placing the intermediate point and of
subdividing from it.

As in this method the intermediate points are placed by
means of the micrometer, there is no inconvenience in placing
the extreme scratches *b* and *β*, &c. at such a distance from
each other, that the intermediate one shall be in no danger
of running into the bur raised by the extremes.

Third Method.

Let *ae* (Fig. 3) be the arch to be quinquesected; lay down
the arches *ab*, *bd*, and *de*, as in the first method; then turn
the beam compass half round, and lay down the arches *α β*

and $\beta \delta$; then, without altering the frame, move the moveable wire of the microscope till it is four times nearer to δ than to e, and, having first rubbed out the former scratches, lay them down again with the compass thus altered; but as this method possesses not much, if any, advantage over the second, in point of ease, and is certainly inferior to it in exactness, it is not worth while saying any thing further about it.

It was before said,* that the center of motion of the beam compass is to be placed, so that the point and axis of the microscope shall both be in the circle in which the divisions are made; but it is necessary to consider this more accurately. Let $A \delta$ (Fig. 4) be the circle in which the scratches are to be made, δ the point of the beam compass, which we will suppose to be exactly in this circle, d the center on which it turns, and $M m$ the wire in the focus of the microscope, and let m be that point in which it is cut by the circle; and let us suppose that this point is not exactly in the line $d\delta$, then, when the beam compass is turned round, the circle will cut the wire in a different point μ, placed as much on one side of $d\delta$, as m is on the other, so that if the wire is not perpendicular to $d\delta$, the arch set off by the beam compass, after being turned round, will not be the same as before; but if it is perpendicular, there will be no difference; for which reason, care should be taken to make the wire exactly perpendicular to $d\delta$, which is easily examined by observing whether a point appears to run along it, while the beam compass is turned a little on its center. It is also necessary to take care that the point δ is in the arc of the circle, while the bisection is observed by the microscope, which may most conveniently be obtained, by placing a stop

on the support on which that end of the beam compass rests.
If proper care, however, is taken in placing the wire perpen-
dicular, no great nicety is required either in this or in the
position of *d*.

Another thing to be attended to, in making the wire bisect
two scratches, is to take care that it bisects them in the part
where they cut the circle; for as the wire is not perpendicu-
lar to the circle, except in very small arches, it is plain, that
if it bisects the scratches at the circle, it will not bisect them
at a distance from it.

There are many particulars in which my description of the
apparatus to be employed will appear incomplete; but as
there is nothing in it which seems attended with difficulty, I
thought it best not to enter further into particulars, than was
necessary to explain the principle, and to leave the rest to
any artist who may choose to try it.

It is difficult to form a proper judgment of the conveniences
or inconveniences of this method, without experience; but, as
far as I can judge, it must have much advantage, both in
point of accuracy and ease, over that of dividing by the com-
mon beam compasses; but it very likely may be thought that
Mr. Troughton's method, is better than either. Whether it
is or is not, must be left for determination to experience and
the judgment of artists. Thus much, however, may be ob-
served, that this, as well as his, is free from the difficulty
and inaccuracy of setting the point of a compass exactly in
the center of a division. It also requires much less apparatus
than his, and is free from any danger of error, from the
difficulty or irregularity in the motion of a roller; in which
respect his method, notwithstanding the precautions used by

Fig. 3.

Fig. 2.
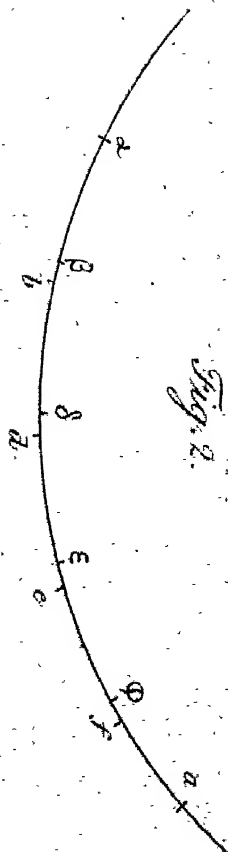
Fig. 1.
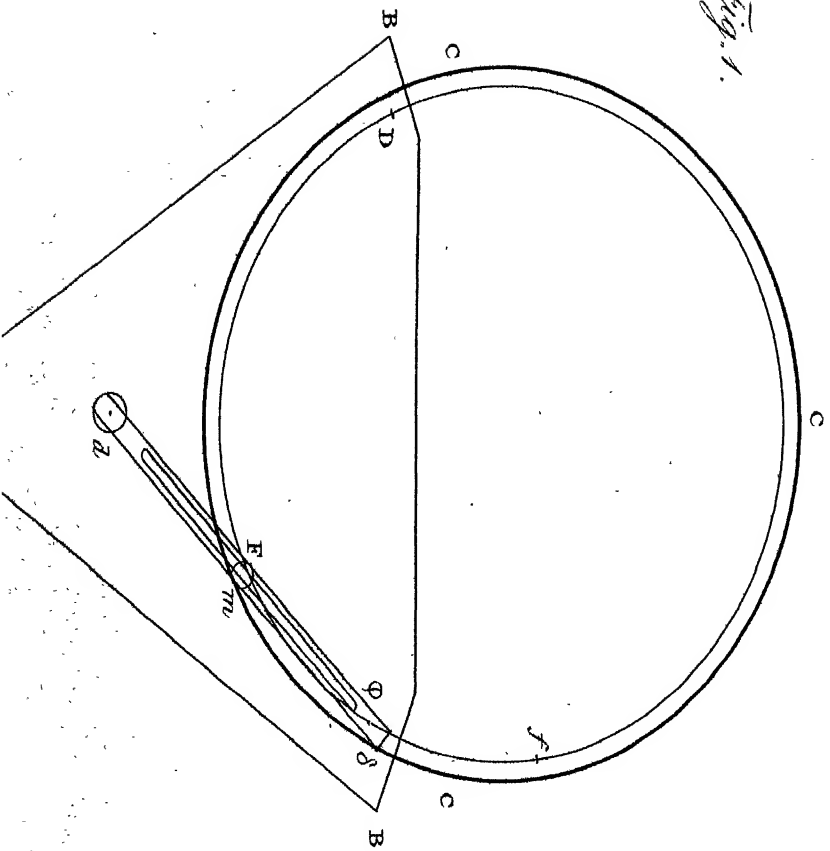
him, is perhaps not entirely free from objection; and what with some artists may be thought a considerable advantage, it is free from the danger of mistakes in computing a table of errors, and in adjusting a sector according to the numbers of that table.

XIV. *On a Method of examining the Divisions of astronomical Instruments. By the Rev.* William Lax, *A. M. F. R. S.* Lowndes's *Professor of Astronomy in the University of Cambridge. In a Letter to the Rev. Dr.* Maskelyne, *F. R. S. Astronomer Royal.*

Read June 1, 1809.

DEAR SIR,

St. Ibbs, Aug. 27, 1808

I AM persuaded that you must feel, in common with myself, how unpleasant it is to make use of an instrument in astronomical observations requiring extreme accuracy, whose exactness you have no adequate means of ascertaining, but are obliged to depend for it in a great measure upon the abilities and integrity of the artist. It is in vain that we observe with so much nicety, and read off with so much precision, if we are still uncertain whether there may not be an error in the instrument itself of much greater magnitude, than those which we are endeavouring to prevent; and that our best instruments must be liable to such errors, no person can possibly doubt, who has paid due attention to the sources from whence they may arise. I have estimated, as accurately as I could, the amount to which they may accumulate in BIRD's method of dividing by continual bisections, and have satisfied myself that they are much more considerable than is generally apprehended; but as I cannot obtain such precise information as I could wish, respecting the exactness with which a bisection can be performed, or a length taken from the scale of equal

parts and laid upon the instrument, I will not trouble you with the deduction which I have made. It is understood indeed, that BIRD's method is now generally laid aside, and that each artist employs one, which he considers in many respects as peculiar to himself; but I presumed that there would still be such a connection betwixt BIRD's method and those which have been substituted in its stead, as to render them in some degree liable to the same errors to which it was subject, and the reports which I have uniformly received from persons, who have had an opportunity of examining some of the modern instruments, have fully convinced me that my opinion was just. But whatever may be the nature of the methods which are now in use, or whatever their advantages over BIRD's, I never could persuade myself that it would be safe to trust to an instrument, without a previous examination. To discover the means of accomplishing this object, is what I have for some time been anxious to effect, and though I fear my endeavours have not been very successful, I will nevertheless take the liberty of presenting you with the result.

You are aware, I believe, that I use a circular instrument for observing both in altitude and azimuth, which was made for me by Mr. CARY in the Strand; that the radius of both the altitude and the azimuth circle is one foot, and that each is divided into parts containing ten minutes. The construction of this instrument does not differ materially from that of other similar instruments, with which you are well acquainted, and I shall not therefore waste your time by giving you a particular description of it. For the purpose of examining the divisions upon the two circles, I procured an apparatus to be prepared by Mr. CARY, which will be very easily explained.

To the face of the rim which surrounds the azimuth circle, and with its left end close to the stand which supports the micrometer on the east side, an arc of brass, concentric with the circle itself, and a little more than 90° in length, an inch in breadth, and one eighth of an inch in thickness, is firmly fixed by screws, so as to have the plane parallel to the plane of the circle, and a small portion of its lower surface resting upon the extreme part of the rim. The screws pass through a brass arc, which is fastened to this at right angles, and lies with its broad side against the face of the rim. Upon the first mentioned arc, a strong upright piece of brass, about six inches in length, is made to slide, the lower part of it embracing the arc as a groove and having a clamping screw underneath, for the purpose of fixing it firmly to the arc at any point required. To the top of the upright piece of brass is attached a microscope, with a moveable wire in its focus, pointing down to the division upon the circle, not directly, however, but with an inclination to the left of about 30°. This inclination is given to it, in order to make it point to the same division upon the circle, which is immediately under the micrometer itself, when it has been moved up as near to the micrometer, as it is capable of approaching. The microscope has attached to it a small graduated circle of brass, and an index, by which the seconds, and parts of a second, moved over by the wire are determined.

To the vertical circle there is likewise an arc applied, of the same length and breadth as the former, but somewhat thicker, and of a radius exceeding that of the circle by about two inches. This greater thickness is given to it, on account of its being supported in a manner which renders additional

strength necessary. It is fixed with its broad convex side downwards upon two brass pillars, screwed fast to the plane of the azimuth circle, and standing in a line parallel to the plane of the vertical circle at the distance of about four inches from it, and on the right side of the pillars which support the micrometers belonging to this circle. The pillar, to which the left end of the arc is fastened, is placed close to the lower micrometer of the vertical circle, and the other contiguous to the elevated rim, in which the divisions of the azimuth circle are cut. The right end of the arc reaches beyond this pillar about ten inches. The pillars are of such a height, and so proportioned to each other, that whilst the left end of the arc, which lies horizontally, is raised to within about two inches of the height at which the lowest point of the vertical circle is placed, the whole arc runs parallel to the circle through an extent of something more than 90°. Upon the arc a microscope, with a moveable wire in the focus, is made to slide as in the former case, and to point to the divisions upon the vertical circle, not directly, but with an inclination of about 30° to the left, in order that the same division (which is the lowest upon the circle) may be seen through it, and through the lower micrometer at the same time.

I will now proceed to shew you, in what manner the examination of the divisions upon either circle may be performed. The process is precisely the same in both cases, and will of course be described in the same words.

The first point to be examined is that of 180°, which must be done in the usual way, by bringing the points of o and 180° to the moveable wires of the opposite micrometers, and then turning the circle half-way round, and bisecting the points

again with the moveable wires; and lastly, taking half the difference betwixt the distances of the wires in the two positions of the circle for the error at the point of 180°. Having now bisected the point of *zero* with the moveable wire of the micrometer, which is intended to be used in the rest of the process (for we shall have no further occasion for both), we must slide the microscope along the arc, till by moving the wire a little we can bisect the point of 90°, and then the micrometer must be firmly clamped to the arc. The circle must then be turned till the point of 180° is brought to the microscope, and that of 90° to the micrometer, so that we may be able to bisect each by a slight motion of their respective wires. This being done, we must observe, from the positions of the wires, how much the interval betwixt them has increased or decreased in the measurement of the new arc; and this increase or decrease must be noted down with a $+$ or $-$ accordingly. In the same manner we must proceed through the remaining two arcs of 90°, observing and noting down the difference betwixt each and the original arc.

The point of *zero* must now be brought again to the micrometer, and bisected by the moveable wire, and the microscope be made to slide back along the arc, till by moving the wire a little we can bisect the point of 60°, and when this is done, the microscope must be clamped. We must then measure the arc of 60° against every succeeding arc of 60° in the circle, precisely in the same way that we measured the first arc of 90° against the other three. The arc of 45° is next to be measured against every succeeding arc of 45°, and this will complete all that is necessary to be done in the early part of the morning before the heat of the sun can have affected the

temperature of the instrument. The rest may be performed at our leisure.

You will immediately perceive the object of this kind of measurement. It enables us to determine, with any degree of accuracy that may be required, the proportion which the first and every succeeding arc of the circle, contained betwixt the micrometer and the microscope, bears to the whole circle, and of course the absolute length of the arcs themselves. Let a denote the real length of the first of these, and $\pm a', \pm a'', \pm a'''$, &c. the difference betwixt the first and second, the first and third, &c. respectively; let A represent any other arc whose length is known, and which is a multiple of a, as marked upon the instrument, and let this multiple be expressed by n. Then will $a + (a + a') + (a + a'') + (a + a''') +$ &c. ... $(a + a''''^{\overline{n-1}}) = A$, and $a = \frac{A - a' - a'' - \dots a''''^{\dots n-1}}{n}$. Hence it is evident, that if there is no error committed in the measurement of any of these arcs, we shall have the value of a, and consequently of $a + a', a + a'', a + a'''$, &c. and of any arc, comprehending any number of these, accurately determined. But if there be an error of e in the measurement of the first, of e', e'', e''', &c. in the measurement of the second, third, &c. respectively, then we shall have the following equation for determining a, viz. $a + (a + a' + e + e') + (a + a'' + e + e'') +$ &c. ... $(a + a''''^{\overline{n-1}} + e + e'''^{\dots \overline{n-1}}) = A$, and consequently a will appear to be equal to $\frac{A - a' - a'' - \dots a'''^{\dots \overline{n-1}}}{n} \frac{-\overline{n-1}e - e' - e'' - \dots e''^{\dots \overline{n-1}}}{n}$, which differs from its true value by $\frac{\overline{n-1}e + e' + e'' + \dots e'''^{\dots \overline{n-1}}}{n}$. Hence it follows, that the value of

the p^{tb} arc (p being greater than unity), as deduced by this process, will differ from its true value by $\dfrac{\overline{n-1}\ e+e'+e''+\ldots e'''\ldots\overline{p-1}}{n}$

$+\dfrac{e''\ldots\overline{p}+\ldots e'''\ldots\overline{n-1}}{n}-e-e'''\ldots\overline{p-1}$, and that if we add any number p of these arcs together, in order to determine the value of the arc which is equal to their sum, we shall have an error in this value (and the expression holds when p is unity, or the first arc only is taken) equal to $p\,\dfrac{\overline{n-1}.e+e'+e''+\ldots e'''\ldots\overline{p-1}}{n}$

$+\dfrac{e''\ldots\overline{p}+\ldots e'''\ldots\overline{n-1}}{n}-\overline{p-1}.e-e'-e''-\ldots e'''\ldots\overline{p-1}=\overline{n-p}.$

$\dfrac{e-e'-e''+\ldots e''\ldots\overline{p-1}+p.e''\ldots\overline{p}+e'''\ldots\overline{p+1}+\ldots e''\ldots\overline{n-1}}{n}.$

Now, if we suppose e to be the greatest error to which we are liable in the measurement of any arc, and each of the succeeding errors to be equal to it, and likewise that $e', e'', \ldots e'''\ldots\overline{p-1}$ are all negative, then it will appear that $\dfrac{n-p}{n}\times 2pe$ will be the greatest error that can be committed in determining the value of any arc, by adding together the values of the (p) smaller arcs of which it is compounded. For instance, if the interval betwixt the micrometer and the microscope comprehends an arc of 60°, as marked upon the instrument, and this arc is measured against every succeeding arc of 60° in the whole circle, we shall have the greatest error that can be committed in deducing the arc of 120° from the addition of the two first arcs of 60°, equal to $\dfrac{6-2}{6}\times 2\times 2e = 2.66e$.

After these remarks, we may proceed to consider how the remaining divisions upon the circle may be examined with the least probable error, and to ascertain the amount of the greatest to which the process can in any case be liable.

Let the arc of 30° be now measured against every succeeding arc of 30° in the first, third, fourth, and sixth arcs of 60°, and let the length of each be determined from a separate comparison with the arc of 60°, in which it is comprehended, and not from a general comparison with all the four. The arc of 15° must then be measured against every succeeding arc of 15° in the first, third, fourth, sixth, seventh, ninth, tenth, and twelfth arcs of 30°, and the value of each deduced from a comparison with the arc of 30°, in which it is contained. When this is done, we shall have determined the length of every succeeding arc of 15°, of the first arcs of 30, 45, 60, 75 ($=60+15$), 90, 105 ($=90+15$), 120 ($=60+60$), 135 ($=90+45$), 150 ($=120+30$), 165 ($=150+15$), and 180° in each semi-circle.

We must next measure the arc of 5° against every succeeding arc of 5° in the whole circle, and deduce the values of the first, and of the sum of the first and second, in each succeeding arc of 15° from a comparison with the arc of 15° in which they are contained. We must then proceed to determine the values of the first arc of 3° in each 15°, and of its multiples the arcs of 6, 9, and 12°. We must also put down the value of the last arc of 3° in each arc of 15°, and then deduce the values of the first and last arcs of 1° in each arc of 15°, from a comparison with the arc of 3° in which they are respectively contained.

We shall now have measured in each arc of 15° the first arcs of 1, 3, 5, 6, 9, 10, 12°, and by taking the last arc of one degree, which has likewise been determined, from the arc of 15°, we shall obtain the first arc of 14°. The first 7° of this arc being measured against the second, we ascertain the value of the first 7°; and then, by measuring the first 4° of the re-

maining arc of 8° against the second, we shall get the value of the first 4°, which added to the arc of 7°, before determined, will give us the length of the first arc of 11°. The first 2° of the remaining arc of 4° must then be measured against the second, and we shall get the value of the first 2°, and by adding this arc to the arc of 11°, we shall obtain the value of the arc of 13°. By taking away the first arc of 1° from the arc of 15°, we get the remaining arc of 14°, and then having determined the length of the first 7° of this arc, by measuring them against the second, we must add it to the arc of 1°, and we shall obtain the arc of 8°. The length of the first 4° of this arc will then be easily known, by measuring them against the second, as will afterwards that of the first 2° in the arc of 4° itself, by measuring them against the second in the same arc.

We have still to ascertain the lengths of all the first arcs of 10, 20, 30, 40, and 50 minutes contained in each degree, for I shall only consider the case in which the circle is divided into parts of 10 minutes. Now the length of the first arc of 30′ will be obtained by measuring it against the second, and the lengths of the first and second arcs of 20′ (whose sum will give the arc of 40′) by measuring the first against each of the remaining arcs. The length of the third arc of 20′ must likewise be put down, and then the first arc of 10′ being measured against the second of the arc of 20′, in which it is included, and also against the two arcs of 10′ contained in the last arc of 20′, its own value, and that of the last 10′ in the degree will be determined from a comparison with the arcs of 20′, in which they are respectively comprehended. The length of this last arc of 10′ being taken from that of the whole degree, will give us the length of the first 50′, and complete the operation.

In order to ascertain the greatest possible error to which we are liable in the examination, let ε denote in parts of a second the greatest that can be committed in bisecting any point upon the limb; then, since this error may occur at each end of the arc, it is evident that e in the expression deduced above $\left(\frac{n-p}{n} \times 2pe\right)$ will become 2ε, and the expression itself $\frac{n-p}{n} \times 4p\varepsilon$. Hence the possible error will be $\frac{2-1}{2} 4\varepsilon = 2\varepsilon$ at $180°$; $\frac{2\varepsilon}{2} + \frac{2-1}{2} \times 4\varepsilon = 3\varepsilon$ at $90°$; $\frac{2\varepsilon}{3} + \frac{3-1}{3} \times 4\varepsilon = 3.33\varepsilon$ at $60°$; $\frac{2}{3} \times 2\varepsilon + \frac{3-2}{3} \times 4 \times 2\varepsilon = 4\varepsilon$ at $120°$. The greatest error must therefore lie betwixt 90 and 120°, and nearer to the extremity of the latter than of the former arc. At 105° it will be 5.50ε; at 111° it will be $5.50\varepsilon - \frac{2}{5}.1,5\varepsilon + \frac{5-2}{5} \times 4 \times 2\varepsilon = 9,70\varepsilon$; and at 111° 10′ it will be $9,70\varepsilon - \frac{1}{6}.1,04\varepsilon$ (the excess of the error at 111° above that at 112°) $+ 3.33\varepsilon = 12.86\varepsilon$, which will be found to be the greatest error betwixt 105 and 120°, and of course the greatest in the first semi-circle. In the other semi-circle, the process being the same, the possible errors must necessarily be the same at the same distances from the first point, reckoning the contrary way upon the circle.

The magnitude of the quantity ε will of course vary upon circles of the same radius, according to the excellence of the glass employed, and the accuracy of the examiner's eye. It will seldom, however, exceed one second upon a circle, whose radius is one foot; and in general it will not amount to so much. I find that I can read off, to a certainty, within less than three fourths of a second, and hence I conclude, that I could examine the divisions of my circle without being liable to a greater error than 9.63 seconds, and those of a circle of

three feet radius without the risk of a greater error than 3,21 seconds.

To those people who are accustomed to entertain such exalted notions of the accuracy with which astronomical instruments can with a certainty be divided, this error, I dare say, will appear very considerable ; but for my part, I am perfectly satisfied that it bears but a small proportion to the accumulated error which may take place, in spite of the utmost vigilance of the artist, in an instrument divided according to any method which has hitherto been made public. I need not, however, remark upon the very great improbability that the error of examination should ever attain, or approach, to its extreme limit, as this must be sufficiently obvious to any person who is in the least degree conversant with the doctrine of chances ; but it may be proper to observe, that we have it in our power (and in this respect the examiner possesses a most important advantage over the divider of an instrument) to diminish its probable amount, as much as we please, by bringing the moveable wires of the micrometer and microscope several times to bisect their respective points in the measurement of every arc, and taking a mean of the different *readings-off* for the true position of the wire at the real bisection of the point. The wire may be moved in this manner eight or ten times at each point (if such a degree of caution should be thought necessary), and the mean taken in little more than a minute, so that the time of performing the work will not be so much increased, as might perhaps have been apprehended, and when it is completed, we may reasonably presume that the distance of every point from zero (whilst the temperature of the circle continues uniform) will have been

determined with sufficient exactness for every practical purpose.

Of the time necessary for the examination, a pretty correct idea may be formed by considering how many measurements are required, and allowing about a minute and a half for each; *i.e.* a quarter of a minute for bringing the extreme points of the arc to the micrometer and the microscope, and a minute and a quarter for making the several bisections. Now, in dividing the whole circle into arcs of 15° each, it will appear that forty-four measurements must be performed; and to examine every point in each arc of 15°, there will be 161 required, making in all 3908 measurements; and consequently the time necessary for completing the whole work will be 5862 minutes, or about 98 hours.

The time and labour required for this examination are, no doubt, very considerable; but it ought to be recollected, that it will render any great degree of precision, in dividing the instrument, totally unnecessary. Whoever indeed employs this method of examinination will be virtually the divider of his own instrument, and all that he will ask of the artist, is to make him a point about the end of every five or ten minutes, whose distance from zero he will determine for himself, and enter in his book to be referred to when wanted. We may likewise observe that by this examination we shall not only be secured against the errors of division, but against those which arise from bad centering, and from the imperfect figure of the circle, and which in general are of too great a magnitude to be neglected.

It will, I dare say, have occurred to you, that whenever we are desirous that an observation should be particularly exact,

we may guard it against the effects of unequal expansion or contraction in the metal, by means of the apparatus which I have described: for we have only to measure the arc which has been determined by the observation against the whole circle, or against the multiple of it, which approaches nearest to the circle, and from thence to deduce its value in the manner explained above, and we shall either have entirely excluded the error which we apprehended, or have rendered it too small to be of any importance. Suppose, for instance, that the arc determined by the observation was 48°; then by measuring it against the whole circumference increased by an arc of 24°, we shall obtain a result free from any greater error of unequal temperature, than one eighth of the increase or decrease of this arc of 24° beyond a due proportion to that of the circle itself.

This expedient gives us all the advantages of the French circle of repetition, without the inconvenience arising from being obliged to turn the instrument, and move the telescope, so many times in the course of the observation. Nay, I am persuaded, that the result may be made more accurate in this way, than by the French method, because not only can the object be more frequently observed, but the contacts or bisections, it may be presumed, will be more exact when the observer is not disturbed by the hurry attendant upon the use of the repeating circle; and with respect to any error in the instrument, from whatever cause it may arise, it will be as effectually excluded by the process which I recommend, as by moving the telescope round the circle. Besides this method is applicable either to the azimuth or altitude circle, or indeed to any circle which turns upon its own axis, whereas

the French method can never be applied to the azimuth circle, nor to any other circle which does not turn both upon its own axis, and upon one which is perpendicular to it.

After all, however, it is possible that the process which I have been explaining to you may be no new discovery, and that you may be already acquainted with it. If this should be the case, you will be kind enough to inform me. At any rate, indeed, I should esteem myself greatly obliged, if you would favour me with your sentiments upon the subject, as soon as you can do it with perfect convenience to yourself.

I am, Dear Sir,

yours, &c.

WILLIAM LAX.

XV. *On the Identity of Columbium and Tantalum.* *By* William Hyde Wollaston, *M. D. Sec. R. S.*

Read June 8, 1809.

WITHIN a short time after the discovery of columbium by Mr. HATCHETT in 1801,* a metallic substance was also discovered in Sweden by M. EKEBERG,† differing from every metal then known to him ; and accordingly he described the properties by which it might be distinguished from those which it most nearly resembled. But although the Swedish metal has retained the name of Tantalum given to it by M. EKEBERG, a reasonable degree of doubt has been entertained by chemists, whether these two authors had not in fact described the same substances ; and it has been regretted that the discoverers themselves, who would have been most able to remove the uncertainty, had not had opportunities of comparing their respective minerals, or the products of their analyses.

As I have lately obtained small specimens of the two Swedish minerals, tantalite and yttro-tantalite, from which I could obtain tantalum, and was very desirous of comparing its properties with those of columbium, Mr. HATCHETT very obligingly furnished me with some oxide of the latter, which remained in his possession.

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* Phil. Trans. for 1802.

† *Vetenkaps Academiens Handlingar.* 1802, p. 68.—*Journal des Mines,* Vol. XII. p. 245.

The resemblance was such in my first trials, as to induce me to endeavour to procure a further supply of columbium, and by application to the Trustees of the British Museum, I was allowed to detach a few grains from the original specimen analysed by Mr. HATCHETT.

Notwithstanding the quantity employed in my analyses was thus limited, I have, nevertheless, by proportionate economy of the materials, been enabled to render my experiments sufficiently numerous, and have found so many points of agreement in the modes by which each of these bodies can or cannot be dissolved or precipitated, as to prove very satisfactorily that these American and Swedish specimens in fact contain the same metal; and since the re-agents I have employed are in the hands of every chemist, the properties which I shall enumerate are such as will be most useful in the practical examination of any other minerals in which this metal may be found to occur.

In appearance the columbite is so like tantalite, that it is extremely difficult to discern a difference that can be relied upon. The external surface, as well as the colour and lustre of the fracture, are precisely the same; but columbite breaks rather more easily by a blow, and the fracture of it is less uniform, appearing in some parts irregularly shattered; nevertheless, when the two are rubbed against each other, the hardness appears to be the same, and the colour of the scratch has the same tint of very dark brown.

By analysis also, these bodies are found to consist of the same three ingredients; a white oxide, combined with iron and manganese.

Either of these minerals, when reduced to powder, is very

readily acted upon by potash; but as the iron contained in them is not affected by alkalies, it appeared better to add a small proportion of borax.

Five grains of columbite being mixed with twenty-five grains of carbonate of potash and ten grains of borax, were fused together for a few minutes, and found to be perfectly incorporated. The colour was of a deep green, from the quantity of manganese present. The mass when cold could be softened with water, and a portion of the oxide could be so dissolved; but it seemed preferable to employ dilute muriatic acid, which, by dissolving all other ingredients excepting columbium, left the oxide nearly white, by the removal of iron and manganese that been combined with it.

The muriatic solution having been poured off and neutralized with carbonate of ammonia, the iron was then separated by succinate of ammonia; after which the manganese was precipitated by prussiate of potash.

The products thus obtained from five grains of columbite, after each had been heated to redness, were nearly,

White oxide	-	4 grains
Oxide of iron	-	$\frac{3}{4}$
Oxide of maganese		$\frac{1}{4}$;

but it cannot be supposed that *proportions* deduced from experiments made on so small a scale can be entirely depended upon, although the *properties* of bodies may be so discerned, nearly as well as when larger quantities are employed.

An equal weight of tantalite taken from a specimen, of which the specific gravity of 7,8, yielded, by the same treatment,

White oxide   - $4\frac{1}{4}$ grains
Oxide of iron   - $\frac{1}{2}$
Oxide maganese $\frac{2}{10}$.

The white oxides obtained from each of these minerals, are remarkable for their insolubility in the three common mineral acids, as both Mr. HATCHETT and M. EKLBERG have observed.

In muriatic acid they cannot be said to be absolutely insoluble; but they are not sufficiently soluble for the purposes of analysis.

In nitric acid they are also nearly, if not perfectly, insoluble.

In sulphuric acid, when concentrated and boiling, the oxide of columbium may be dissolved in small quantity, and so also may the oxide obtained from tantalite.

The proper solvent, as has been observed by Mr. HATCHETT and by M. EKEBERG, is potash; and as it is not required to be in its caustic state, I employed the crystallized carbonate of potash on account of its purity and uniformity. Of this salt about eight grains seemed requisite to be fused with one of the oxide obtained from either of these minerals to render it soluble in water.

Soda also combines with the oxide, and may be said to dissolve it; but a far larger proportion of this alkali is necessary, and a larger quantity of water. And although a solution may have been effected that is transparent while hot, it very soon becomes opaque in cooling, and finally almost the whole of the oxide subsides combined with a portion of the soda in a state nearly insoluble.

When a solution of the white oxide, obtained from either of these minerals, has been made, as above, with potash, the whole may be precipitated by the addition of an acid, and will not be redissolved by an excess of sulphuric acid, of nitric, of muriatic, succinic, or acetic acids.

But there is a further agreement in the properties of these two minerals, which appears above all others to establish their identity; for though they are both so nearly insoluble by any excess of the mineral acids, yet they are each completely dissolved by oxalic acid, by tartaric acid, or by citric acid; and the solution of each is subject to the same limitations; for if the precipitate has been dried, it is become intractable, and can scarcely be dissolved again till after a second fusion with potash.

If to the alkaline solution of either of them there be added infusion of galls, prussiate of potash, or hydrosulphuret of potash, no precipitate occurs; but when a sufficient quantity of acid has been added to neutralize the redundant alkali, the infusion of galls will then occasion an orange precipitate; but prussiate of potash causes no precipitate, nor does the hydrosulphuret precipitate the oxide, although the solution may become turbid from precipitation of sulphur by a redundant acid.

The characteristic precipitant of columbium is consequently the infusion of galls; but in the employment of this test certain precautions are necessary. For as an excess of potash may prevent the appearance of this precipitate, so also may a small excess of oxalic or tartaric acids prevent precipitation, or dissolve a precipitate already formed. A larger excess of citric acid seemed requisite for that purpose, and would also dissolve the gallat of columbium. In each case the precipi-

tate may be made to appear by neutralizing the redundant acid; and for this purpose carbonate of ammonia should be employed: for although pure ammonia has no power of dissolving the oxide alone, yet the gallat seemed to be perfectly re-dissolved by that alkali.

When infusion of galls is poured upon the white oxide recently precipitated, and still moist, it combines readily and forms the orange-coloured compound.

Prussiate of potash occasioned no change in an oxide that had been purified by a second fusion with potash; but it appeared to dissolve a small portion of the oxide, as infusion of galls, poured into the clear liquor, occasioned a cloudy precipitate of an orange colour, though no such precipitate took place when the infusion was mixed with the same prussiate alone.

Hydrosulphuret of potash being added to the oxide, and heated upon it, impaired the whiteness of its appearance, and seemed to detect the remains of some impurity which had not yet been removed by other means; but no appearance indicated the formation of a sulphuret of columbium.

From a careful repetition of these experiments upon each of the oxides, I see no reason to doubt of their perfect agreement in all their chemical properties; but there is nevertheless a very remarkable difference in the specific gravities of the two minerals from which they are extracted.

The specific gravity of columbite was ascertained by Mr. HATCHETT to be 5,918; that of tantalite was found by M. EKEBERG to be 7,953, and I have every reason to suppose their results correct, since a small fragment of the former appeared upon trial to be 5,87, while a specimen of tantalite,

weighed at the same time, was as much as 7,8. I should, however, observe, that the specific gravities of three other fragments borrowed for this purpose were not so high, that of one being 7,65, of another 7,53, and of a third so low as 7,15.

It is evident that no variation of mere proportion of the ingredients can account for an increase of specific gravity from 5,918 to 7,953, which are in the ratio of 3 to 4; for since columbite contains four fifths oxide, if the whole remaining one fifth part in weight of that oxide could be supposed added to the same bulk, without diminution of the quantities of iron and manganese, the specific gravity would not then exceed 7,1; and even if a weight equal to one third of the whole were thus added, without increase of bulk, still the aggregate would not quite equal the heaviest tantalite in specific gravity; but, on the contrary, the quantity of white oxide in this specimen certainly does not amount to six sevenths, and probably is not more than five sixths of the whole mass.

The only chemical difference, by which this circumstance could be explained, would be the state of oxidation, which my experiments cannot appreciate; but it may also arise in part from actual cavities in the mass of columbite, and in part from the state or mode of aggregation.

XVI. *Description of a reflective Goniometer.* *By* William Hyde Wollaston, *M. D. Sec. R. S.*

Read June 8, 1809.

From the advances that have been made of late years in crystallography, a very large proportion of mineral substances may now be recognised, if we can ascertain the angular dimensions of their external forms, or the relative position of those surfaces that are exposed by fracture. But though the modifications of tetrahedrons, of cubes, and of those other regular solids, to which the adventitious aid of geometry could be correctly applied, have been determined with the utmost precision, yet it has been often a subject of regret, that our intruments for measuring the angles of crystals are not possessed of equal accuracy, and that in applying the goniometer to small crystals, where the radius in contact with the surface is necessarily very short, the measures, even when taken with a steady hand, will often deviate too much from the truth to aid us in determining the species to which a substance belongs.

A means of remedying this defect has lately occurred to me, by which in most cases the inclination of surfaces may be measured as exactly as is wanted for common purposes, and when the surfaces are sufficiently smooth to reflect a distinct image of distant objects, the position of faces only $\frac{1}{50}$ of

an inch in breadth may be determined with as much precision as those of any larger crystals.

For this purpose, the ray of light reflected from the surface is employed as radius, instead of the surface itself, and accordingly for a radius of $\frac{1}{50}$ of an inch, we may substitute either the distance of the eye from the crystal, which would naturally be about twelve or fifteen inches; or for greater accuracy we·may, by a second mode, substitute the distance of objects seen at a hundred or more yards from us.

The instrument which I use, consists of a circle graduated on its edge, and mounted on a horizontal axle, supported by an upright pillar (Plate XI). This axle being perforated, admits the passage of a smaller axle through it, to which any crystal of moderate size may be attached by a piece of wax, with its edge, or intersection of the surfaces, horizontal and parallel to the axis of motion.

This position of the crystal is first adjusted, so that by turning the smaller axle, each of the two surfaces, whose inclination is to be measured, will reflect the same light to the eye.

The circle is then set to *zero*, or 180°, by an index attached to the pillar that supports it.

The small axle is then turned till the further surface reflects the light of a candle, or other definite object to the eye; and, lastly, (the eye being kept steadily in the same place) the circle is turned by its larger axle, till the second surface reflects the same light. This second surface is thus ascertained to be in the same position as the former surface had been., The angle through which the circle has moved, is in fact the supplement to the inclination of the surfaces; but as

the graduations on its margin are numbered accordingly in an inverted order, the angle is correctly shewn by the index, without need of any computation.

It may here be observed, that it is by no means necessary to have a clean uniform fracture for this application of the instrument to the structure of laminated substances; for since all those small portions of a shattered surface, that are parallel to one another (though not in the same plane), glisten at once with the same light, the angle of an irregular fracture may be determined nearly as well, as when the reflecting fragments are actually in the same plane.

In this method of taking the measure of an angle, when the eye and candle are only ten or twelve inches distant, a small error may arise from parallax, if the intersection of the planes or edge of the crystal be not accurately in a line with the axis of motion;* but such an error may be rendered insensible, even in that mode of using the instrument, by due care in placing the crystal; and when the surfaces are sufficiently smooth to reflect a distinct image of objects, all error from the same source may be entirely obviated by another method of using it.

For this purpose, if the eye be brought within about an inch of the reflecting surface, the reflected image of some distant chimney may be seen inverted beneath its true place, and by

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* I cannot omit mentioning, that Mr. SOWERBY had thought of employing reflection for this purpose, nearly at the same time as myself; but did not succeed to his satisfaction, in consequence of an attempt to fix the position of the eye. For when the line of sight is determined by a point connected with the apparatus, the radius employed is thereby limited to the extent of the instrument, and the error from parallax is manifestly increased.

turning the small axle may be brought to correspond apparently with the bottom of the house (or with some other distant horizontal line.) In this position the surface accurately bisects the angle, which the height of that house subtends at the eye (or rather at the reflecting surface); then, by turning the whole circle and crystal together, the other surface, however small, may be brought exactly into the same position; and the angle of the surfaces may thus be measured, with a degree of precision which has not hitherto been expected in goniometry.

The accuracy, indeed, of this instrument is such, that a circle of moderate dimensions, with a vernier adapted to it, will probably afford corrections to many former observations. I have already remarked one instance of a mistake that prevails respecting the common carbonate of lime, and I am induced to mention it, because this substance is very likely to be employed as a test of the correctness of such a goniometer, by any one who is not convinced of its accuracy from a distinct conception of the principles of its construction.

The inclination of the surfaces of a primitive crystal of carbonate of lime is stated, with great appearance of precision, to be $104° 28' 40''$: a result deduced from the supposed position of its axis at an angle of $45°$ with each of the surfaces, and from other seducing circumstances of apparent harmony by simple ratios. But however strong the presumption might be that this angle, which by measurement approaches to $45°$, is actually so, it must nevertheless be in fact about $45° 20'$; for I find the inclination of the surfaces to each other is very nearly, if not accurately $105°$, as it was formerly determined

to be by HUYGENS;* and since the measure of the superficial angle given by Sir ISAAC NEWTON † corresponds with this determination of HUYGENS, his evidence may be considered as a further confirmation of the same result; for it may be presumed, that he would not adopt the measures of others, without a careful examination.

IN THE ANNEXED PLATE,

a b. Is the principal circle of the goniometer graduated on its edge.

c c. The axle of the circle.

d. A milled head by which the circle is turned.

e e. The small axle for turning the crystal, without moving the circle.

f. A milled head on the small axle.

g. A brass plate supported by the pillar, and graduated as a vernier to every five minutes.

h. The extremity of a small spring, by which the circle is stopped at 180°, without the trouble of reading off.

i i and *k k.* Are two centers of motion, the one horizontal, the other vertical for adjusting the position of a crystal: one turned by the handle *l*, the other by the milled head *m.*

The crystal being attached to a screw-head at the point *n* (in the center of all the motions), with one of its surfaces as

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* HUYGENII Opera Reliqua, Tom. I. p. 73 —Tract. de Lumine.

† NEWTON's Optics, 8vo. p. 329. Qu. 25, concerning Iceland Crystal.

nearly parallel as may be to the milled head m, is next rendered truly parallel to the axis by turning the handle l till the reflected image of a horizontal line is seen to be horizontal.

By means of the milled head f, the second surface is then brought into the position of the first, and if the reflected image from this surface is found not to be horizontal, it is rendered so by turning the milled head m, and since this motion is parallel to the first surface, it does not derange the preceding adjustment.
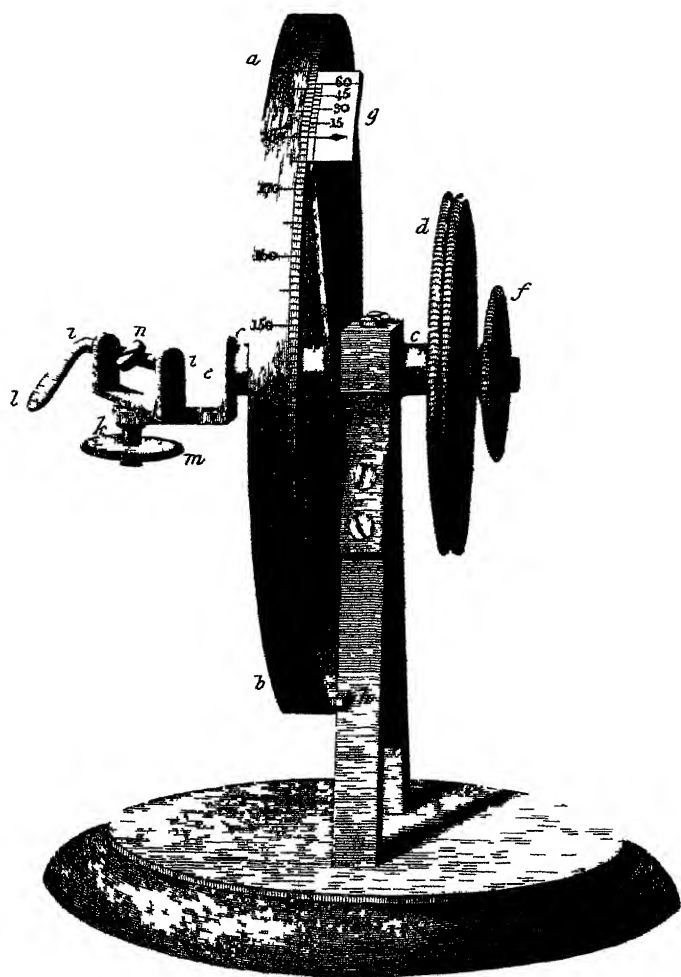
XVII. *Continuation of Experiments for investigating the Cause of coloured concentric Rings, and other Appearances of a similar Nature.* *By* William Herschel, *LL. D. F. R. S.*

Read March 23, 1809.

In the first part of this paper, I have pointed out a variety of methods that will give us coloured concentric rings between two glasses of a proper figure applied to each other, and it has been proved that only two surfaces, namely, those that are in contact with each other, are essential to their formation; it will now be necessary to enlarge the field of prismatic phenomena, by showing that their appearance in the shape of rings has been owing to our having only used spherical curves to produce them.

35. *Cylindrical Curves produce Streaks.*

As soon as it occurred to me, that the cause of the figure of any certain prismatic appearance must be looked for in the nature of the curvature of one or both of the surfaces, that are essential to its production, I was prepared to expect that if a spherical curve, when applied to a plain surface of glass, produces coloured rings, a cylindrical one applied to the same would give coloured lines or streaks. To put this to the proof of an experiment, I ground one side of a plate of glass into a cylindrical curve, and after having given it a polish, I laid a slip of plain glass upon it, and soon perceived a beautiful set of

coloured streaks. The broadest of them was at the line of contact, and on each side they were gradually narrower and less bright. The colours in the streaks were similar to those in the rings, and they were in the same manner changeable by pressure as in them. Their order was likewise the same, if we reckon from the line of contact, as with rings we do from the center; so that these streaks differed in no respect from rings, except in their linear instead of circular arrangement.

When the cylindrical surface was laid upon a plain slip of glass, the same streaks were seen as in the former experiment. They were of a lively red and green colour, and I saw at least ten, eleven, or twelve on each side of the line of contact.   ·

Metalline surfaces had the same effect, for when the cylindrical surface of glass was laid on a plain metalline mirror, I had red, orange, yellow, green, and blue streaks. In the same manner a plain slip of glass placed upon a polished part of a brass cylinder of $3\frac{1}{2}$-inch in diameter, produced also coloured streaks.

The combination of two cylindrical surfaces has an effect on the streaks, which is similar to that which the contact of two spherical ones has on the rings; for when I placed the cylindrical surface of glass longitudinally upon the polished part of the brass cylinder, the streaks were contracted as rings would have been by the application of two spherical curves to each other.

36. Cylindrical and spherical Surfaces combined produce coloured elliptical Rings.

The theory which suggests to us that the particular figure of every prismatic appearance between glasses depends on the curvature of the surfaces which are in contact, is still farther confirmed when spherical and cylindrical curves are applied to each other; for these, accordingly, should give elliptical rings; and when I tried the experiment, by laying a 26-inch double convex lens upon the cylindrical surface of my plate of glass, it produced a coloured elliptical central part, encompassed with gradually vanishing rings of the same figure. By changing the focal length of the lens, I could alter the proportion of the conjugate to the transverse axes of these elliptical rings at pleasure. A lens of 55 inches gave ellipses that were much flattened, and one of 5 inches gave them nearly circular.

37. Irregular Curves produce irregular Figures.

The modifying power of surfaces may be further established by such as have no regular figure; for these ought to give irregular prismatic phenomena, and this was fully proved by the following experiment.

I took a large piece of mica which had a very glossy but irregular surface, and when a 34-inch double convex lens was placed upon a small ridge of it, several pretty straight streaks might be seen, but wherever the ridge was waving the streaks were following the same direction. In some places the mica gave irregular, coloured arcs, that were concave to some distant centre; and in others, the various contorted figures, that

were to be seen, exceeded all the imaginary forms which the most inventive fancy can paint. The flexibility of mica also gave room for using different degrees of pressure, by which means a continual change of figure and succession of prismatic colours was produced.

When I laid a piece of this mica upon a cylinder, and placed a plain slip of glass or double convex lens upon it, all its irregularities were modified into disfigured streaks with the former, and distorted ellipses with the latter.

Experiments of a similar nature were made upon the irregular surface of Island crystal and other substances, which all gave the same result.

38. *Curved Surfaces are required for producing the coloured Appearances at present under Consideration.*

It has already been seen, in the first part of this paper, that spherical curves give circular rings, and I have now shown that cylindrical forms produce streaks; that a combination of spherical and cylindrical curvatures give elliptical rings, and that all sorts of variegated coloured phenomena are made visible by surfaces, which are irregularly and variously curved; these experiments prove in the fullest manner that the curvature of surfaces is the cause of the appearance, as well as of the shape of the coloured phenomena which are produced. For if we can invariably predict, from the nature of the curves we employ in an experiment, what will be the appearance and form of the colours that will be seen, it certainly must prove the efficacy of these curvatures in the production of such phenomena. This will receive additional confirmation in the following article, which shows that

39. *Coloured Appearances cannot be produced between the plain Surfaces of two parallel Pieces of Glass applied to one another.*

As the production and modification of the figure of the coloured appearances, that have hitherto been considered, has in the last article been ascribed to curved surfaces, it will be necessary to examine whether such phenomena may not also be seen between the plain surfaces of two parallel pieces of glass applied to each other directly in contact, or inclined towards each other in some certain extremely small angle.

The latter of these cases has already been considered in the 31st article of the first part of this paper, where I have shown that two plain surfaces, let the angle of the wedge of air between them be as small as you please, will not give coloured streaks. I have indeed seen two thin plain pieces of glass, with a slip of platina of an extraordinary thinness between them at one end tied together, which showed some streaks near the place where the glasses were in contact, but when I removed the thread that bound them together, the streaks vanished, which proves that the glasses had been constrained, and thus had probably assumed some curvature at the point of contact.

I have also tried two flat surfaces of glass, which were so perfect that no colour could be perceived unless they were by unequal pressure somewhat disfigured, and when that was the case large flashy coloured appearances became visible, and their configuration followed very evidently the stress which I laid upon the different parts of the glasses.

It is however unnecessary to dwell on proofs, that streaks cannot be seen when two plain parallel pieces of glass are

applied to each other, as it will hereafter be shown that when the incumbent plain glass is not of a parallel thickness, coloured phenomena may be rendered visible between two perfectly plain surfaces, although no force or strain should be used to produce a fallacious, curved, contact.

40. *Of the Production of coloured Appearances.*

Hitherto I have only considered the coloured rings which Sir Isaac Newton has pointed out, and have shown, at the end of the 28th article, that no more than two surfaces are essential to their formation. It has now also been proved, that the configuration of the coloured phenomena arises from the curvature of one or both of the two essential surfaces. From these principles it will be seen, that we are to distinguish between the production of the colours and that of their configuration when produced. By the experiments that have been given, the cause of the configuration is laid open to our view; but the production and arrangement of the colours remain to be investigated.

The leading feature of the arrangement of the colours of the rings is prismatic; that is to say their order is red, orange, yellow, green, blue, indigo, and violet; in order, therefore, to enter minutely into the subject, I shall have recourse to some prismatic experiments.

It will be necessary here to mention, that the proposed enumeration of the modifications of light, which was intended to have been given in this part of my paper, is grown to such an extent by the number of experiments I have made upon the subject, that its introduction would occasion a long interruption of the present subject; and although undoubtedly the

action of bodies and surfaces on light would be better understood, if all the modifications wherein colours are produced had been before us, yet as the experiments I have to relate may be made plain, either by referring to modifications that are sufficiently known, or by explaining what is not already familiar, I shall postpone the intended enumeration to some future opportunity, and confine myself at present to a few remarks relating to them.

The colours contained in white light may be separated by reflection, as well as by refraction, and what is perfectly to my present purpose, the order, in which the colours thus produced are arranged, is the same in both cases; each of these principles therefore may cause coloured appearances, which the particular figure of the surfaces we use will mould into different configurations.

Sir ISAAC NEWTON, for instance, has shown that the rays of light will be separated, by what he calls a different reflexibility, when they fall on the base of a prism; the violet being reflected first, and the red last.* By this property of the differently coloured rays, he has explained a very remarkable phenomenon, which is that in a prism, when exposed in the open air, and when the eye is properly placed " the spectator will see a bow of a blue colour."† From the little the author has said of this bow, it may be supposed that he did not examine it farther than was required for his purpose; it will therefore be necessary to enter more fully into the subject.

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* See the illustration of the 9th experiment in the first book of NEWTON's Optics, page 46.

† See the 16th experiment in the second part of the first book, page 145.

41. *Particulars relating to the Newtonian prismatic blue Bow.*

The Newtonian blue bow may very conveniently be examined, when a right angled prism is laid down on a table before an open window. The eye being then brought to a convenient altitude, and pretty near the side of the prism, we see in it a bow, which from the predominant colour may be called blue. It contains some green followed by blue, indigo, and violet. A very faint red, orange, and yellow may also be perceived above the greenish colour; but these belong not to the blue bow, and have not been noticed by the author. Their appearance will hereafter be accounted for.

To analyse this blue bow more particularly, let us admit that the colours, which give it the general appearance of what may be called blue, consists of half the green, and of all the blue, indigo, and violet rays, which are reflected while the other half of the green, the yellow, orange, and red are transmitted. Then the angle of obliquity, at which this separation of the colours will happen, in consequence of the different refrangibility of the differently coloured rays assigned by NEWTON, will be $49° 46' 12'',5$.

Let A B C D E, Plate XII. Fig. 1, be rays of light moving within glass in such directions, as to fall on the interior base F G upon the points $\alpha \beta \gamma \delta \varepsilon$. Then, if it be required that these rays after reflection from the base should meet in the point H, and form the blue bow, the angles $A \alpha G$, $B \beta G$, $C \gamma G$, $D \delta G$, and $E \varepsilon G$ must be respectively equal to $49° 46' 12'',5$; $49° 49' 20''$; $49° 55' 33'',6$; $49° 59' 41'',4$; and $50° 7' 54''$; which will give the angles $\alpha H \beta$, $\beta H \gamma$, $\gamma H \delta$, $\delta H \varepsilon$, equal to $3' 7'',5$, $6' 13'',6$, $4' 7'',8$, and $8' 12'',6$, making in the whole

the angle subtended by the bow $\alpha\,H\, \epsilon\, 21'\,41'',5$.* For in consequence of the different reflexibility of the differently coloured rays the violet, indigo, blue, and faintest half of the green rays will be reflected between α and β, if they fall on that space in any angle between the above mentioned ones contained between $A\,\alpha\,G$ and $B\,\beta\,G$; and will therefore meet at H, and form the greenish blue part of the bow. The red, orange, yellow, and the brightest half of the green rays, on the contrary being less reflexible, will be transmitted through the base between α and β, and by refraction pass in proper angles into the air. The letters $v\,i\,b\,\frac{1}{2}g$, which in the figure are placed within the space $\alpha\,H\,\beta$, denote the reflected colours, and $\frac{1}{2}g\,y\,o\,r$ put under the base between α and β are the initials of the transmitted colours; and in the same manner the reflections and transmissions which must happen between $\beta\,\gamma$, $\gamma\,\delta$, and $\delta\,\epsilon$ are expressed by the letters over the base for the former, and under it for the latter. The order of the colours of the blue bow, when it is seen at H, is perfectly explained by the letters in the reflected part; and the eye must be placed, for seeing it, at the mean obliquity between the angles $A\,\alpha\,G$ and $E\,\epsilon\,G$, which is $49°\,57'\,3'',3$.

In order to conform this account of the blue bow, to the manner in which it was viewed by NEWTON, I have preserved his way of ascribing the separation of the rays to their different reflexibility, which however is merely the effect of their

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* There is a mistake in one of the angles given by NEWTON, when in his Optics, page 145, he explains the blue bow; for 49 deg. $\frac{1}{28}$ taken from 50 deg. $\frac{1}{3}$, makes the breadth of the bow 1° 4' 31'',4, which contradicts the refractions he has given, page 112. As he only takes in the blue, indigo, and violet colours, instead of $49\frac{1}{28}$ degrees, it should rather be $49\frac{23}{28}$.

different refrangibility. The angles at which the rays that constitute the blue bow are separated from the rest, may very properly be called *critical*, and the effect, which is the consequence of the oblique incidences that have been given, may with equal propriety be called a *critical separation* of the differently coloured rays of light.

42. *Account of a prismatic red Bow.*

I must now introduce a prismatic appearance, which on account of its similarity with the Newtonian blue bow, from which it only differs in colour, I have called, a prismatic red bow. It consists of red, orange, yellow, and some green rays; and the red colour being upon the whole very predominant, it may not improperly be called a red bow. It is not produced by the Newtonian different reflexibility of the differently coloured rays of light, but owes its origin to a modification which takes effect at the outside of the prism at very oblique angles of incidence, and may be called a different intromissibility; but this, like the Newtonian different reflexibility, is only the consequence of the different refrangibility of light.

To see the red bow, an observer should place himself in the open air, and standing with his back within a few feet of some wall or building, hold the side of an equilateral prism flat over his eyes, and look upwards to an altitude of about 80° at the heavens; he will then see a beautiful arch of a deep red colour, succeeded by a bright orange and yellow, with a considerable portion of green on the inside. The comparative darkness of the building behind will show the light in front to the best advantage. It is also to be observed, that all

experiments on prismatic bows succeed best, when the heavens are totally overcast with an uniform cloudiness.

To analyze the production of this bow, let A B C D E, Fig. 2, Plate XII. be rays of light moving in air, in such directions as to fall on the exterior base FG, of a piece of glass, upon the points $\alpha \beta \gamma \delta \varepsilon$; then, if it be required that these rays, after their intromission into the glass should meet in the point H and form the red bow, the angles A α H, B β H, C γ H, D δ H, and E ε H, must be respectively equal to 130° 29′ 33″,6 ; 133° 40′ 33″2 ; 134° 29′ 28″2 ; 135° 36′ 13″2 ; and 136° 10′ 38″,0 ; from which we have the angles A β B, A γ C, A δ D, and A ε E, which a red ray would make were it to pass out of glass into air, equal to 3° 15′ 45″,5; 4° 7′ 30″5; 5° 19′ 17″5; and 5° 56′ 50″,5. Now by the laws of the different refrangibility of light, the red rays are intromissible at α, when by refraction they make the angle H α F $=$ 49° 30′ 26″4 ; but the orange cannot be intromitted any where between α and β with any effect on the red bow, since it is only at β, where the angle H β F is 49° 35′ 12″3, that they can enter the glass so as to come to the eye at H. The yellow rays will, for the same reason, be efficiently intromitted only at γ, where they will make the angle H γ F 49° 38′ 2″,3, and the brightest half of the green rays will find an efficient entrance from δ to ε, since the smallest angle of their intromission H δ F is 49° 43′ 4″,3, and the angle H ε F, which terminates the red bow, is 49° 46′ 12″5. The arrangement of the colours of this bow will be seen, as it was in the blue bow, from the letters placed above the base, which denote those that are intromitted so as to come to the eye; the rest of the colour-making rays, which cannot come in that direction, being marked by letters placed under the base. The

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whole angle of the red bow α H ϵ is 15′ 46″,1, and the mean obliquity of the eye at H is 49° 38′ 19″,5.

In the calculation of both the bows, the situation of the eye at H has been determined, as it would be, were the rays to remain in glass; but as they will be refracted by the side of a prism, when they come out of it, proper computations must be made not only of the place of the eye in air, but also of the angle which the bow will subtend; for this will be found to be considerably different in different prisms; those that have large refracting angles will magnify the bows more, and require the eye to be nearer than others that have smaller angles.

These bows may be examined at leisure, by projecting them upon a white ground in the following manner:

In a dark room, by a reflecting apparatus, I admitted a horizontal beam of the solar light through an opening of about an inch and a half in diameter. The formation of the bows requiring scattered light,* I covered the opening with a piece of glass evenly roughned on both sides. Then, with an intention to obtain a projection of the blue bow, I placed a prism having one angle of 91° and the other two nearly equal, close to the emeried surface, and turned it upon its axis till the angle of obliquity of the scattered rays, that fell on one side of the prism, was proper for the required critical separation of the coloured rays. The obliquity of the middle ray with the base, for this purpose, it has been shown, must be 49° 57′ 3″,3. In this position the interior critical separation of the prismatic colours taking place, the blue part, namely the violet, indigo, blue, and about one half of the green rays were

* See the first paragraph of the 46th article of this paper.

reflected, and passing through the opposite side of the prism projected the blue bow upon the cieling of the room. The colours may there be conveniently seen; but as this bow is composed of the least luminous rays of the prismatic spectrum, it requires considerable attention to perceive the faintest of them. The green and blue are most visible, and by receiving the bow upon a screen of white paper held at the most favourable distance, the fainter colours, when the illumination is very bright may also be perceived.

In order then to project also the red bow, I turned the prism upon its axis till the scattered light fell with a proper obliquity on the base of it; the angle required for this purpose, it has been shown, must be from $0^{\circ}\ 0'\ 0''$ to $5^{\circ}\ 56'\ 50''5$; the side of the prism, which is turned towards the opening, should be covered with a slip of pasteboard to prevent any light from entering it. In this situation, I saw a very bright arch containing red, orange, and yellow projected at some distance backwards upon the cieling; that part of the green which no doubt was also transmitted, was lost in the brightness which is to be seen within the bow, for the same reason that the faint colours of the blue bow can only with great difficulty, if at all, be perceived; namely, that they join the dark inside of the bow. For NEWTON has proved that the space beyond the convex part of the blue bow must be bright, and that beyond the concave dark; but in the red bow, as my theory will show, we have that on the convex dark, and on the concave bright. This experiment therefore proves, that here, by the gradual intromission of the differently coloured rays, a critical separation takes place on the outside of the prism, similar to that which by reflection happens in the blue bow at the in-

side; and by which, in the present case, the red part of the prismatic spectrum, that is, the red, orange, yellow, and some of the green, can only reach the eye.

43. *Of a sudden Change of the Colours of the Bows.*

It has been shown that the red bow should be seen nearly in the same place where the Newtonian blue bow is visible. For in the 41st article the place of the eye, for seeing the blue bow in the prism of 100 degrees, was determined to be at an obliquity of 49° 57′ 3″,3; and with the red bow, and in the same prism, it has been shown that the eye must be placed at the obliquity of 49° 38′ 19″,5. The difference is only 18′ 43″,8, and by the following experiments it will be found, that both the bows may actually be seen nearly in the same part of every prism; and that the direction of the light, by which we see either the blue or the red bow, determines which of the two will be visible. To prove this, let a right angled prism be laid down on a sheet of white paper before a window, and when the eye is placed in the proper situation for seeing a re-flected blue bow, we may instantly transform it into a trans-mitted red one, by covering the side of the prism which is towards the incident light with a slip of pasteboard; for by stopping the direct light, which before fell on the base of the prism, and was there reflected, we then see the bow by light intromitted from the paper through the base, which, as has been explained, will be red.

With proper management we may have the bow half red and half blue; blue in the middle with red sides, or red in the middle with blue sides; which appearances it will not be required to explain any farther, especially after what has

already been said in the 18th article of the first part of this paper of the change of the colours of rings.

When we have before us a bow that is half blue and half red, it will be seen that both taken together contain all the prismatic colours in their regular order of refrangibility. It will now also appear that the faint red, orange, and yellow, which I have said are to be perceived above the blue bow* may arise either from an imperfectly transmitted red bow, which always lies concealed under the Newtonian blue one, or perhaps more probably from the partial reflection of the red, orange, and yellow rays, many of which will come to the eye notwithstanding they are also copiously transmitted.†

According to my account of the red bow, it ought to be seen in the prism a little above the blue one, and this is also further confirmed by any one of the experiments in which we have some part of each bow in view at the same time, for then the relative situation of the two bows will be visible.

Similar experiments may be made by candle light upon either of the bows; for when a sheet of white paper is pinned against a wall, that it may reflect the light of a candle placed upon a table about three or four inches from the paper, we may then see the blue bow in a prism placed upon a dark ground before the reflecting paper; and the green colour, which it is not very easy to perceive distinctly in daylight, will here be very visible, and the more so if we use an equilateral

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* See the first paragraph of the 41st article.

† In my modifications of light I have proved, by undeniable experiments, that within a prism as well as on the outside of it the rays of all the colours are equally reflexible, and that a critical separation of them only takes place at those angles where by refraction a ray cannot be transmitted.

prism instead of a right angled one. When the reflecting paper is removed from the wall and laid under the prism, that the light may then be thrown upwards and transmitted through the base, we see a bow of a lively red colour.

Before I can introduce more intricate phenomena, it will be necessary to advert to some other particulars relating to these bows.

44. Of Streaks and other Phenomena produced from the prismatic blue and red Bows.

It has been remarked in the 40th article, that the production of colours and their configuration when produced are owing to different causes; this will now be confirmed by an experiment.

Scattered rays, when they fall on a prism will by a critical separation of the colours, produce both the blue and the red bows, and these coloured appearances when produced may be modified into streaks, circular rings, and other forms, by the configurating power of surfaces. When a plain glass or metalline mirror is laid under the base of a right angled prism in which we see the blue bow, the contact of the two plain surfaces will immediately produce a great number of coloured streaks. They will be found to be parallel to the bow, most of them within and some just under it. They may be seen without any lens, merely by looking into the prism with the eye pretty close to the surface through which we see the blue bow. This experiment proves that plain surfaces, though they cannot produce colours, have a power of modifying and multiplying them when produced. As I shall have occasion hereafter to be more particular, I shall now only mention that when we lay a

spherical surface, such as an object glass, under the prism it will immediately give us several sets of innumerable concentric coloured rings ; and, as will now be readily expected, a cylindrical surface placed under the prism will give a number of lenticular appearances, such as are contained between the intersections of two circular arches drawn concave towards each other. The irregular surface of mica will in like manner produce multiplications of appearances, that may be seen much better than they can be described.

When the same surfaces are applied to the red bow, phenomena that are perfectly of the same form will be made visible within and just under the bow ; and the streaks will also be in a parallel direction.

The side of the prism, to which a plain glass must be applied, is of singular use in the explanation of many appearances of the coloured phenomena, which are to be seen, and it is on this account that the formation of the generated colours into all sorts of configurations has been noticed before I come to that part of this paper, wherein this subject must find a further discussion ; for by the application of a slip of plain glass, we can decisively ascertain the nature of any coloured appearance in the prism. Thus, when we see a common coloured red or blue arch, occasioned by the mere different refrangibility of light, the plain glass any how applied to the prism will give no streaks. If we apply the plain glass to a transmitting side, we can have no streaks from a critical blue bow, because it is occasioned by reflection ; and for the same reason, when the plain glass is applied to a reflecting side, we can have no streaks that belong to a critical red bow, because it originates at the intromitting surface. With the assistance of

this criterion I may now proceed to a review of more compli-
cated phenomena.

45. *Explanation of various Appearances relating to prismatic*
Bows.

If, in the open air, we look into the zenith with a right
angled prism held across the eyes, we shall see two red bows
convex towards each other. They are caused by the bright
transmissions of the light of the heavens through the sides in
which the bows appear; for when to either of these sides the
criterion of the plain glass is applied, we shall have coloured
streaks. The course of the rays which produce the two bows is
delineated in Fig. 3, Plate XII. ABC represents the prism, and
the rays that can enter the eye when they fall on A B within
the limits $a\,b$ A from $0^\circ\,0'\,0''$ to $5^s\,56'\,50'',5$, which are the red,
orange, yellow, and the brightest part of the green, will form
the red bow ; and the situation of the eye at E will be had by
the mean refrangibility of the rays which give the bow; for
as the angle Bcd must be $49^\circ\,38'\,19'',5$, we have the obliquity
B$dc = 85^\circ\,21'\,40'',5$ and the angle CeE that conveys the ray
to the eye will be $82^\circ\,49'\,34'',2$. The same thing will happen
on the other side of the prism, where the rays $m\,n\,o\,p\,q$ will
come to the eye at E, in an equal but differently directed angle
BqE, and cause an inverted red bow to be seen in the side
A C.

When we look down into the side of an equilateral prism
we see a blue bow, but on lifting the eye and prism gently
up together towards the zenith, the bow, at a certain altitude,
will be changed from blue to red; and by the application of
the criterion, it is proved that we see the first by reflection,

and the last by transmission. For, suppose ab, Fig. 4. Plate XII. to be a ray of a mean refrangibility between the violet, indigo, blue, and half the green; when this falls on the side A C of the equilateral prism A B C with an obliquity ab A of $57° 58' 28'',5$, it will be refracted so as to make the angle C cd $70° 2' 56'',8$ which gives $49° 57' 3'',3$ for the angle C dc; and consequently the ray def E will come to the eye by the same angles of reflection and refraction as it entered the prism, and make AfE equal to A ba. The eye at E will therefore see a blue bow. Then if a plain glass be applied to the transmitting side A C there can be no streaks; for blue bows being caused by the critical separation of the rays occasioned by the Newtonian reflexibility, the plain glass must be in contact with the reflecting side; and as soon as we hold it against B C, the coloured streaks will make their appearance. The change of the colour of the bow, on lifting the prism and eye together towards the zenith, is represented in figure 5; for the light from the sky, which will enter the prism on the side A B, will eclipse the blue bow which was seen before by light entering from the ground through the side A C in figure 4; then if ab fig. 5 is a ray of the mean refrangibility of the red bow, it will by refraction give the angle Bcd $49° 38' 19'',5$, from which we obtain Bdb equal to $70° 21' 40'',5$, and the ray will, by a second refraction, come to the eye in an angle CeE of $58° 44' 12'',4$, where the red bow will be seen; but in order to produce coloured streaks, the plain glass must now be applied to the transmitting side A B.

When a right angled prism is held in the hand, so that the light of the sky through an open window may fall upon the base, if then an observer with his back to the light looks through

the base into the side A C of the prism A B C fig. 6, he will see an erect blue bow by two reflections, only one of which however is the cause of the critical separation of the coloured rays, the other being a common one. For when a mean refrangible blue-bow-ray falls with an obliquity abC of $82°\ 17'\ 31''$ on B C, it will by refraction give the angle $Bcd = 85°\ 2'\ 56'',8$, from which we obtain $Ade = 49°\ 57'\ 3'',3$, which being the mean angle of the critically separated rays, they will by reflection pass to the side A C, where the angle of the common reflection Cef will be $40°\ 2'\ 56'',8$; this gives $efB\ 85°\ 2'\ 56'',8$, and by refraction the middle of the blue bow will be seen by an eye at E in an angle EgB equal to the angle abC. From the construction of the figure, it is evident that the eye may be drawn from E towards a, and always keep the blue bow in view, which will still remain erect; for when the eye comes to a, the rays by which the bow is seen will then enter at E, and the critical reflection will still remain at d, as may be satisfactorily proved by an application of the plain glass to AC, which will cause no streaks, whereas they will immediately appear when it is held under the side A B.

When the eye looks into the side B C with the same obliquity of $82°\ 17'\ 31''$, but differently directed, so that in fig. 7 the angle may be abB, instead of abC a blue bow will again be seen, but in an inverted position. This also may be drawn over into the other side of the prism without an alteration of its appearance, the reason of which is sufficiently evident from the construction of the figure; but in this case the critical reflection will be a e, and the common one at d.

It will be proper to shew that like appearances of the red bow may be seen; for this purpose let the prism be laid with

one side upon a sheet of white paper placed in a window, with the base towards the observer, as represented in fig. 8. In this position, the light from without reflected by the paper under the prism will be brighter than that from within the room, and the very oblique incident rays $a\,b$ will be refracted by the horizontal side A B, so as to make the angle B $c\,d$ equal to $49°\ 38'\ 19'',5$, from which we have B $d\,c = 85°\ 21'\ 40'',5$, and by refraction C e E $= 82°\ 49'\ 34''2$, the eye placed at E will therefore see an erect red bow in the horizontal side A B, which may be drawn over into the perpendicular side without change of position; for the scattered rays reflected from the paper will also enter the prism in the same oblique angle of incidence from the opposite direction $a\,b$ fig. 9; where having caused the red bow by an intromissive critical separation at c, they will come to the eye after a common reflection from the side A C, in the same angle as before.

When an inverted red bow is to be seen the eye must be placed a little lower, and the calculation of the angles in the 10th and 11th figures, which represent the course of the rays, being similar though differently directed, will be sufficiently understood by an inspection of them; but as in fig. 8 and 9, the intromissive separation was produced by the horizontal side, so it is, in these figures, effected by the vertical one; all which may be proved by a proper application of the criterion.

There are many other phenomena attending the bows, but as they are more intricate, and not necessary for my present purpose, I leave them to the ingenuity of those who have entered into the preceding calculations, which are quite sufficient to point out the method that should be taken for explaining them.

46. *The first Surface of a Prism is not concerned in the Formation of the blue Bow, nor of the Streaks that are produced by a plain Glass applied to the efficient Surface.*

It has already been mentioned that the bows are formed by scattered light; but to have a direct experimental proof that such light, if not absolutely necessary to the formation of the bows, is at least equally efficient with regularly refracted light, I took a prism with one side of it roughened on emery, and receiving the light through it when the eye was in the situation required for seeing the blue bow, I saw it as completely formed by scattered light, as it could have been by light regularly refracted through a polished side.

A natural consequence of this experiment seems to be, that the form of the surface through which light enters can be of no consequence; this will however admit of a more convincing proof, as follows: upon the middle of the side of a right angled prism, through which the rays entered that caused the blue bow, I laid a plano convex lens of an inch and a half focus; the result was, that not the least alteration could be perceived either in the form or in the colour of the bow, both which remained as perfect under the place where the incident rays passed through the lens as they were on each side of it. When I changed the convex lens for a plano-concave glass of the same focus, appearances were still the same; and when by a critical application of a plain glass I produced coloured streaks from the base of the prism, the interposition of either the convex or concave glass was equally immaterial. A scattering glass applied to the incident ray, had no other effect than to diminish the brightness of the bow.

The same experiment may be repeated with the red bow; but as here the first surface is essential to the formation of the bow, the plain side of the convex lens or concave glass, when placed against the prism, as before, will produce streaks; neither the bow, nor its streaks however will be in the least affected by the convexity or concavity of the outward surface of the glass applied, through which the light is admitted. A scattering glass will have no effect to disturb the bow or its streaks, and when this glass is emeried on both sides, we have again the bow complete, but without streaks; and by this fact it is proved, that unless a polished plain reflecting surface is applied to the prism, streaks cannot be formed.

47. *The Streaks which may be seen in the blue Bow contain the Colours of both the Parts of the prismatic Spectrum, by the critical separation of which the Bow is formed.*

The most favourable way of observing the colours of the blue bow streaks that are formed when a plain glass is laid under the base of a right angled prism, is to place a screen of white paper, before an open window, and to let the direct solar light shine through it upon the side of the prism. This scattered light will be bright and uniform, and cause no adventitious colours to mix with the streaks. The eye should be within six or seven inches of the prism. A streak consists of a certain principal colour and the intermediate tint which separates it from the next; and in the following memorandum of fourteen streaks, which I saw in the manner above described, the principal colours are placed in front, and the dividing tints at the side between them.

1. Very faint blue,

          - - - - - Pale red.

2. Faint blue,

          - - - - - - Pale red.

3. Blue,

          - - - - - - Pale red.

4. Bright blue,

          - - - - - Faint red.

5. Purple blue,

          - - - - - Whitish red.

6. Bluish red,

          - - - - - Whitish red.

7. Deep red,

          - - - - - - - Greenish white.

8. Red,

          - - - - - - - Greenish white.

9. Red,

          - - - - - - - Pale bluish green.

10. Red,

          - - - - - - - Pale bluish green.

11. Pale red,

          - - - - - - Pale bluish green.

12. Paler red,

          - - - - - - Dirty white.

13. Dingy yellow,

          - - - - - Dirty white.

14. Dingy yellow.

To ascertain whether the second surface of the subjacent glass, which by other experiments I know to have a multiply-

ing power of at least six or seven reiterated interior reflections, all of which may be seen through the side of the prism, had any share in the production of these streaks, I fixed on one side of it a glass, of which the lowest surface was emeried, and on the other a metalline plain mirror, but found that the streaks were both in number and colour perfectly alike in them all.

By this account it is evident that the streaks derived from the blue bow contain not only the colours of the blue reflected, but also those of the red transmitted part of the spectrum. This fact is a clear indication of the office which is performed by the surface of the subjacent plain glass, which is simply that of reflecting back the rays of the transmitted red part of the spectrum, which being mixed with the blue part, both together, by their intersections, produce the observed streaks, as will be explained hereafter.

That the colours of the transmitted part of the spectrum are reflected back into the prism, is a point which I suppose will be admitted ; but if it should be imagined that the red rays in the streaks of the blue bow might come into the prism by a scattered reflection of the light which falls on the plain glass under its base, then I say that a sheet of white paper or double emeried glass, ought to give the brightest streaks ; whereas, on the contrary, neither of them produces any ;* it is therefore evident, that a regular reflecting surface is necessary to their formation ; but such a surface, be it glass or metal, can only reflect red rays when it receives them ; and since we know that the red part of the spectrum is transmitted, and must fall on the reflecting surface, it is but fair to conclude that the

* See the last paragraph of the preceding article.

rays, of which that part is composed, are those which by re-flection re-enter the prism.

48. *On the Formation of Streaks.*

As I have now ascertained that the streaks we see when a plain glass is laid under a prism, which shows the blue bow, are formed by the principle of reflection, which throws back the transmitted rays, it will be a considerable satisfaction if we can trace the course of these rays far enough to have some idea of the arrangement, whereby such appearances may be produced. To show, by calculation, the complete formation of the streaks in a case that is liable to such variation, on account of the different contact between the modifying surfaces, the position of the light and the inclination of the eye, would be a most laborious, if not endless, undertaking; it will there-fore be sufficient, if I can make it appear, that streaks must unavoidably be produced by the rays which after transmission are reflected back again, and mix with those that form the bow; and this I believe will not be difficult. For instance, let F G, fig. 12, Plate XIII. be the base of a solid piece of glass, in which a compound ray of light is moving from A to α, with an obliquity $A \alpha G = 49° 46' 12,''5$; and let I K be the plain sur-face of a reflecting substance placed under the base; then will the violet, indigo, blue, and the faintest part of the green of this ray be reflected at α, and the remaining green, the yellow, orange, and red will be transmitted. Now, in order to un-derstand the intention of this figure, it will be necessary to observe that on account of the minuteness of the operations of light, all the lines and distances are represented upon a scale one thousand times larger than what the calculation gives

them. The real dimensions of several lines therefore cannot find room in the figure, and must be supplied by imagination. The distance of the eye from the base F G, for instance, which in the calculation has been assumed to be only three inches, will be 3000; the diameter of the pupil of the eye 200; the breadth of the base not less than 2160; and the subtense of the whole blue bow will be twenty-four inches eight tenths. The distance between the reflecting surface I K, and base F G, I have supposed to be the ten thousandth part of an i ıch; it is therefore in this figure represented by one tenth of an inch, and the space $\alpha\beta$, in which the colours that have been mentioned are transmitted, and which by calculation is ,003588 is expressed by 3,59 inches.

The rays of the different colours which are transmitted at α will be refracted in different angles, and when they come to the reflecting plane will be returned to the base in such a direction, as to come to it again in the same angle in which by refraction they left it; but their distance from the point α, when they reach the base, will differ considerably. If we call the angle of refraction φ, and the distance of the reflecting plane from the base x, then $2x \times \frac{\text{rad.}}{\text{tan. } \varphi}$ will be an expression for the intervals at which the several rays will re-enter the base, which for red will be $\alpha r = $,0019198, for orange $\alpha o = $,0022974, for yellow $\alpha y = $,0026675, and for green $\alpha g = $,0043053. At these places the rays will be a second time re-fracted, and rise towards the eye in parallel directions, and with an obliquity of 49° 46′ 12″,5 equal to that of their incidence A α G. Their course is represented in the figure by the letters $\alpha\, r\, r'\, r''$, $\alpha\, o\, o'\, o''$, $\alpha\, y\, y'\, y''$, and $\alpha\, g\, g'\, g''$.

These things being premised, I proceed to explain the consequences that must arise from the mixture of the transmitted with the originally reflected rays. The first is, that the rays which after transmission re-enter the prism at different points, and are the cause of the streaks, will not proceed in a parallel direction with those that by reflection from the same or neighbouring points form the blue bow. For instance, let A $\alpha\, \alpha'$, and B $\beta\, \beta'$, fig. 13, be two incident and reflected rays of the blue bow; then if the yellow ray transmitted at α after two refractions, and one reflection, not expressed in this figure, re-enters the prism at y, it will make the angle $y'\, y$ F equal to the angle A α G. But from the construction of the blue bow, it has been shown that B β G is greater than A α G; $\beta'\, \beta$ F is therefore greater than $y'\, y$ F, and the rays $\beta\, \beta'$ and $y\, y'$ will meet somewhere in the line $\beta\, \beta'$ produced. If we call the greatest of the two angles m, the smallest n, and the distance of the angular points d, then $d \times \dfrac{\text{sin. } n}{\text{sin. } m - n}$ will give us the length of the line $\beta\, \beta'$, at which the two rays will meet and intersect each other, which according to the enlarged size of this figure, will be at 773 inches from β. For the same reason the orange ray $o\, o'$ will meet $\beta\, \beta'$ at 1084 inches, and the red ray $r\, r'$ at 1401 inches from β. It follows also from the same construction, that some of the transmitted rays will diverge from the reflected ones; for instance, the green ray transmitted at α, which re-enters the prism at g, will make the angle $g'\, g$ F less than the angle $\beta'\, \beta$ F; the rays $\beta\, \beta'$ and $g'\, g$ will therefore diverge. To this may be added, that $g\, g'$ $y\, y'$ $o\, o'$ $r\, r'$ and $\alpha\, \alpha'$ will be parallel.

If such difference between the directions of the transmitted

and reflected rays takes place, it will be seen that the rays transmitted through different points are among themselves subject to the same variety in the direction of their course; $r' r''$, $o' o''$, $y' y''$, $g' g''$, for instance, which passed through the point a, are parallel to each other; but all of them converge respectively to $r r'$, $o o'$, $y y'$, $g g'$ transmitted through c; and on the other hand $y' y''$, $o' o''$, $r' r''$, diverge from $g g'$.

Fig. 14, Plate XIV. is a general representation of the course of the rays of the blue bow, and of those that produce the streaks. The base of the bow is divided into twenty equal parts, and one ray of the bow reflected from each of the points of the division is marked by a line. Twenty-one sets of rays of the different colours transmitted through the same points re-enter the base at their calculated places, and are represented by dotted lines drawn at proper angles; but here it should be noticed, that the difference of the twenty angles being much too small to give any idea of their converging or diverging condition, the difference between each set has been expressed by one degree less towards the right, and one degree more towards the left; the angle of the middle ray being of its proper magnitude. The strong lines marked A a, B β, C γ, D δ, E ϵ, show the division of the colours, and are the same which in fig. 1 were used to explain the construction of the blue bow. The rays incident on the base FG, in the direction of these lines, which are reflected in the same angles, and are also marked with strong lines, meet at the point where the eye is supposed to be placed.

The figure has been drawn by the result of a strict calculation contained in the following table. In the first column are the angles of the obliquity of the incident rays; in the second

we have the distances of the reflecting points on the base
from α. The remaining columns contain the distances also
reckoned from α, at which the transmitted rays of the seve-
ral colours re-enter the base, after two refractions and one
reflection.

Green.	Blue.	Indigo	Violet.
,0043053			
,0049503			
,0057881			
–			
,0067327	,0118750		
,0077443			
–	,0122780		
,0088005	,0115764		
,0098887	,0112212		
,0110005			
,0121302	,0131457	,0188450	
–	,0141088	,0183685	
,0132741		,0186142	
,0144294	,0151325		
,0155939	,0161973		
,0167661	,0172916		
–	–	,0204987	,0231720
,0179447	,0184081	,0196009	
,0191288	,0195415	,0214808	
–	,0206884	,0225168	,0231409
,0203177	,0218460	,0235906	,0236904
,021516	,0230'25		,0244812
,0227070	,0241864	,0246915	,0253990
,0239066	,0253664	,0258130	,0263933
,0251090	,0265518	,0269504	,0274380
,0263138			

Table of Calculati[on]

No.		Obliquity.	Distances.	Red.	Orange.	Yellow.
1	α	49 46 12,50	,0000000	,0019198	,0022974	,0026675
2		49 47 17,58	,0012397	,0030962	,0034310	,0037464
3		49 48 22,65	,0024794	,0042782	,0045780	,0048491
4	β	49 49 27,73	,0037191	,0054653	,0057356	,0059726
5		49 50 32,80	,0049588	,0066566	,0069021	,0071117
6		49 51 37,88	,0061985	,0078517	,0080759	,0082630
7		49 52 42,95	,0074382	,0090501	,0092559	,0094243
8		49 53 48,03	,0086779	,0102514	,0104413	,0105938
9	γ	49 54 53,10	,0099176	,0114553	,0116311	,0117701
10		49 55 58,18	,0111573	,0126614	,0128249	,0129523
11		49 57 3,25	,0123397	,0138695	,0140221	,0141395
12		49 58 8,33	,0136367	,0150797	,0152224	,0153309
13		49 59 13,40	,0148764	,0162915	,0164254	,0165261
	δ	49 59 41,00	,0154406	—	—	—
14		50 0 18,48	,0161161	,0175048	,0176307	,0177246
15		50 1 23,55	,0173558	,0187194	,0188382	,0189259
16		50 2 28,63	,0185955	,0199354	,0200476	,0201299
17		50 3 33,70	,0198352	,0211525	,0212588	,0213301
18		50 4 38,78	,0210749	,0223707	,0224715	,0225144
19		50 5 43,85	,0223146	,0235899	,0236857	,0237545
20		50 6 48,93	,0235543	,0248100	,0249013	,0249663
21		50 7 54,00	,0247794	,0260309	,0261180	,0261797

.From the complex nature of this figure, it will immediately be seen that we cannot attempt an investigation of the particular streaks, that will be formed by the mixture of the transmitted with the reflected rays. An inspection of it, however, will be sufficient to show that streaky appearances must be produced. For instance, between α and the first red ray which re-enters the base, a narrow blue streak should be seen; this will be broken in upon by the mixture of two sets of red, orange, and yellow rays, which together with the reflected colours of the bow, the green being still wanting, must give a pale red division immediately joining the blue streak. When we advance farther into the figure, the great mixture of the colours and the different directions of the rays are so various, that nothing particular can be determined without entering into a very complicated calculation of the meeting and intersections of the rays; we see, however, that these mixtures will produce a condensation of rays in some parts, and vacancies in others, so that no uniform tinge can remain, and consequently streaky appearances must be seen. The same conclusion may be drawn from an inspection of the places where the transmitted colours re-enter the base; for the green, which is transmitted between α and β does not enter again till after the fourth division of the base; the blue which begins to be transmitted at β cannot find admittance again till after the tenth; the indigo transmitted from γ to δ does not re-enter into the composition till after the sixteenth division; and the violet transmitted between δ and ϵ will only come in again after the nineteenth. There will consequently be a considerable space without green, another without blue, a third without indigo, and a fourth without violet; from

which it follows, that streaky appearances must every where be seen in the composition of the rays that come to the eye. We should also notice that towards δ all colours but violet will be transmitted, for which reason when they rise again a compound of them will produce streaks that approach to white, such as pale red, pale bluish green, dingy yellow, and dirty white; so that both at the beginning and end of the bow-streaks all observations * of them agree perfectly with what is pointed out by the foregoing remarks; and though we have not analysed the particular construction of the streaks in the middle of the bow, yet what has been said will sufficiently prove that various successive changes of the colours must also take place.

It will be understood that I have only attempted to give some idea of the action of surfaces, in giving configuration to colours that are already produced; but that the principle of reflection is the cause of streaks will remain evident, even if the method of its action should not have been explained so much to our satisfaction as we might wish. It will also remain to be proved, that streaks are only the effect of one of those modications which depend on the figure of the reflecting surface; † and having got thus far in this research, I may * advance towards a final consideration of my subject.

49. *Prismatic Bows when seen at a Distance are straight Lines.*

The next point to be shown, in order to approach gradually to a solution of my problem, is that the apparently arched figure of the blue and red bows, which may be seen in a prism,

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* See the first paragraph of the last article.

† See the second paragraph of the 44th article.

is merely the consequence of the position of the eye, and the modifying power of the surface through which it sees them. For a proof of this, it would be sufficient to refer to the principles of the formation of the bows, from which it must be evident that the critical separation of the rays will be exerted in every direction, and that the extent of the bows we see would consequently be parallel to the sides and base of the prism, if the eye could receive the rays which form them, every where in the same angle from a line drawn parallel to that side of the prism through which they pass. An experimental confirmation of this we have by laying down a prism, and keeping either of the bows in view while we gradually draw the eye away; it will then be seen that the curvature, which the bows had assumed, will continually be diminished, and nearly vanish at a very moderate distance.

50. *The Colours of the Bow-streaks owe their Production to the Principle of the critical Separation of the different Parts of the prismatic Spectrum.*

That streaks will be produced when a plain glass is laid under the side of a prism which forms either of the coloured bows, has already been sufficiently shown; but that these streaks, as well as the rest of the phenomena which have been mentioned in the 44th article, are exclusively to be deduced from the same principle by which the bows have been explained will require some proof. With regard to streaks, the following experiment, I believe, will remove every doubt upon the subject.

Let a plain glass be laid under the base of a right angled prism; then, if the eye at first be placed very low, no streaks

will be seen; but when afterwards the eye is gradually elevated, till by the appearance of the blue bow we find that the principle of the critical separation of colours is exerted, the streaks will become visible, and not before; nor will they remain in view when the eye is lifted higher than the situation in which the effects of the critical separation are visible. It is therefore evident, not only that the colours are furnished by the same cause which produces the bow, but also that they are modified into streaks by the plain surface under the prism.

In addition to this, it must be remarked that the criterion, which has been successfully used in the explanation of several prismatic phenomena, proves that no other colours, but those which arise from the same source, can be modified so as to give streaks. The following experiment will show the foundation on which this criterion is established.

Let there be an horizontal opening in the upper part of a window-shutter, of about three feet long and one foot high; then, if we look at it through one side of a right angled prism, we shall see a red bow from the highest margin of the opening, and a blue one from the lowest; but when a plain glass is applied to either of the sides of the prism through which we see these bows, neither of them will give any coloured streaks. The experimenter must carefully keep the critical bows out of the way; for should either of them fall upon those which are under examination, streaks must of course be seen to pass over them.

When a spherical surface is placed under the prism, it has likewise been shown that coloured rings will be seen; but these, like the streaks, will not be visible when the eye is

below the place where the bows can be seen, which would not have happened had a plain glass been used instead of the prism; for with such an arrangement, coloured rings may be seen at the most oblique as well as perpendicular stations of the eye.—As soon as the blue bow is perceived, the rings begin to be formed, first partly, then half, and lastly, we see them completed; and what is remarkable, these coloured rings are of such a magnitude and brightness, that they cannot be a moment mistaken for those we see when a plain glass is laid upon the same spherical curve.—The eye being then gradually elevated above the range in which the bows may be seen, these rings will pretty suddenly shrink in their dimensions, and lose much of their brilliancy; till at last, when the eye comes to a perpendicular situation, we find them dwindled away to the size and appearance of such as may be seen when a plain glass is substituted for the prism.

Irregular surfaces are no less decisive in the phenomena they exhibit; for when an equilateral prism is laid upon red mica in a strong illumination of scattered light, we may see a most admirable variety of very minute coloured appearances, whenever the eye is brought to the blue bow place; but as soon as it is in the least elevated above, or depressed below that situation, these fantastical figures are sure to vanish.

51. *A Lens may be looked upon as a Prism bent round in a circular Form.*

Those who have followed me in the analysis of the blue and red bows, will readily enter into the application I shall make of this theory to the generation of coloured rings by lenses.

It has been proved, that the different refrangibility of the prismatic colours, at certain critical angles, will cause the violet; indigo, blue, and part of the green rays to be separately reflected, and that, according to what has been said in ths 49th article, this will produce an extended straight-lined appearance tinged with the abovementioned colours. It has also been shown that the same principle, at certain critical angles, will cause the red, orange, yellow, and part of the green rays to be exclusively intromitted, in such directions as will produce a similarly extended straight-lined appearance tinged with these latter colours. From the angle in which the eye must receive these appearances in a prism, they are converted into the blue and red bows; but, since they would appear to be straight lines, if they were seen in directions perpendicular to a line drawn parallel to the edges of the prism, it follows, that were a long prism bent round into a circular form so that its two ends might meet, these lines would then be changed into rings, one of which would be formed by reflection, the other by transmission.

A lens may be said to be such a prism, from which indeed it differs only in one respect, which is, that an angle contained between two lines applied as tangents to different parts of its surface is changeable, whereas the refracting angle of a given prism is constant.

If it should be remarked that in consequence of considering a lens in this light, a plano-convex one, for instance, ought to present us, in certain situations, with a ring of the colours of the blue bow, and in others with a similar ring containing those of the red one, I must observe that the reason why such rings or bows can never be seen by the eye, though the phy-

sical separation of the rays should actually take place, is owing to that particular circumstance in which, we have remarked, the lens differs from a prism, namely, the curvature of the refracting surface; for although it has been proved that the figure of the first surface of a prism is not concerned in the formation of the blue bow, yet that of the surface through which it is seen by the eye is of material consequence, as will appear by the following experiment.

An equilateral prism, one side of which I had made cylindrical, was exposed so as to receive the incident light through the convex surface. In this situation, the eye being about three or four inches from the prism, a bow was formed which in every respect was like one I saw in another equilateral prism, whose three sides were flat; but when the convexity of the first prism was turned towards the eye, the bow could no longer be seen, although the critical separation of the rays would undoubtedly form it in this, as well as in the other prism ; the two sides and angles of each exposed to the light being perfectly equal. By much attention to what may be perceived when the eye is placed at various distances, I found that the curvature of the surface through which I tried to see the bow, produced a focal contraction and subsequent inversion of the rays in their passage to the eye, and thus occasioned a total change of appearances. Now, since a ring or bow would not be visible in a prism bent round, if the side through which it must be seen were curved, we cannot expect to see such appearances in a lens, which every where presents us with a spherical surface.

The effect upon the appearance of the bows, produced by the surface through which the rays must pass to come to the

eye, may be still better examined by laying the plain side of a plano-convex glass of a short focus upon the flat side of a prism, through which we see either of the bows; for when the eye is near the focus of the lens, they will be entirely effaced as far as they are covered by the lens.

A consequence of great importance may be drawn from these experiments; for since the cause of the coloured appearances, which have been called bows when seen in a prism, is now perfectly understood to be the critical separation of the colours of the incident light, it must be admitted that such a separation will certainly take place whenever a beam of light can find an entrance into glass, so as to make the required angles either with an interior or exterior surface, be it in the shape of a prism, lens, or solid of any kind, although the figure of the last transmitting surface should not permit such coloured-appearance-making-rays to reach the eye. A plano-convex lens will consequently by its construction separate the rays of light which enter at the convex surface in such a manner, as by reflection to produce what, if it could be seen, would be called a blue bow, and by rays that come in at the plain side, separate them by intromission so as to produce a red one.

To remove all doubt about the truth of this theory, I ground a small part of a plano-convex lens flat, that I might look into it, as it were, through a window, to see what passed within. The flat made an angle with the base of about thirty-four degrees, and I saw through it very plainly, in different directions of the illumination, a blue bow by light entering at the convex surface, and a red bow by light coming in at the plain one.

With regard to a plain glass contained between parallel

surfaces, it may be remembered that when in the last paragraph of the 39th article I said that streaks could not be seen by laying another plain glass under it, I intimated at the same time the formation of colours; this will now admit of a satisfactory explanation. Scattered rays will enter into a parallel piece of glass, and by reflection the critical separation of colours will take place on its interior surface, so that if this effect could be seen, a blue bow would appear; and in the same manner a red bow might be seen by rays intromitted through the lowest surface. In consequence of the course of these rays, streaks would also appear from each of the bows when another plain glass is laid under the parallel piece; but from a calculation made according to the principles that have been established in the preceding part of this paper, the reflection of a mean ray of the blue bow from the interior surface being at the angle 49° 57′ 3″,3; and this being also the oblique incidence on the upper surface, a ray which comes in that direction with the mean refrangibility of the rays of the blue bow cannot come out of glass. The angle of obliquity of the mean intromitted ray for the red bow is 49° 38′ 19″,5, and on computing its direction by the mean refrangibility of the red bow, it will also be found that it cannot clear the glass. I have seen the bows and their streaks when the upper surface of the glass was inclined only nine degrees to the lower one; and possibly a much smaller angle would have been sufficient to permit the emergence of the coloured rays. The strong reflection from the outside of the glass, and the contraction of the dimensions of the bows are however much against perceiving them at a great obliquity.

52. The critical Separation of the Colours, which takes place at certain Angles of Incidence, is the primary Cause of the Newtonian coloured Rings between Object-glasses.

It has been proved that streaks, concentric rings, lenticular figures, and all sorts of irregular coloured phenomena may be seen by means of the prism ; and in the 35th, 36th, and 37th articles, it has already been sufficiently explained that the cause of the great variety of these appearances is to be found in the configurating power of surfaces. I have also remarked in the 40th article, that in order completely to account for the Newtonian rings, it remained only to be shown how the colours thus modified are produced.

The prismatic experiments contained in this paper have explained in what manner a critical separation of the colours, which takes place at certain angles of incidence, is the cause of the appearance of the blue and red bows ; since the different reflexibility of the rays of light, by which NEWTON has accounted for the blue bow, brings on a critical separation of the blue colours, and since also the different intromissibility by which I have explained the red bow, occasions an equally critical separation of the red ones.

In the 50th article I have not only proved that all the above described various appearances, which in the first part of this paper were produced by convex glasses, may be equally well obtained by the use of a prism, but have also shown that the great simplicity of this valuable optical instrument has cleared up great difficulties, by pointing out to us that the colours which are modified into such various shapes, are in all prismatic experiments exclusively produced by the critical separation

of the rays of light. Now, as this must be admitted; it will certainly not be philosophical to look for a different cause of the same or similar effects, when convex glasses, which have all the required prismatic properties are used to produce them.*

To show the great similarity, or rather the identity of these effects, let us examine them in different points of view, and since the variety of the configurations is no longer an object that wants explaining, I shall only take the most simple case of each, namely, the coloured rings, that are produced when a plano-convex lens is laid with its convex side upon a plain reflecting surface; and the coloured streaks which are produced when the base of a right angled prism is in the same manner placed upon such a surface.

The form of rings arises from the spherical figure of the lens.†

The right-lined appearance of the streaks is owing to the straight figure of the plain surface of the prism.‡

The colour of the rings may suddenly be changed. §

The colour of the blue bow-streak may as instantly be converted into those of the red bow. ||

The cause of the sudden change of the rings has been shown to be that the sets of one colour are seen by reflection, and those of other by transmission.¶

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* By this it will be understood that if any case should occur, in which the critical separation cannot account for the observed phenomena, we are then authorised to look out for some other cause to explain them.

† See the first paragraph of this paper.

‡ See the 49th article.

§ See the 15th article of the first part of this paper.

|| See the 43d article.

¶ See the 8th article of the first part of this paper.

It has also been shown that the blue bow-streaks are seen by reflection, and those of the red bow by transmission. *

In a lens we may at the same time see, in half the set, the colours of the reflected, and in the other half, the colours of the transmitted rings.†

And in a prism held before an open window, when the eye is close to it, and when half the bow falls on the side of the room, we may see blue streaks by reflection from half the blue bow, and green streaks by transmission from half the red bow. ‡

When deep convex, or concave glasses, are laid upon the first surface of a lens, the rings are not affected by it.§

And when the same glasses are laid upon the first surface of a prism the streaks remain unaltered. ‖

When the convexity of the lens, which is placed on the reflecting surface, is changed, the size of the rings is also changed.¶

And when the angle of the prism is increased or diminished, the distance of the streaks undergoes a proportional alteration. **

When the lens is pressed upon the plain glass, the rings increase in diameter.††

And by a pressure of the plain glass against the prism the distance of the streaks grows larger.

To form rings by a lens, scattered light is only required.‡‡

And the same light is best for the production of streaks by a prism. §§

Many other instances of similarity might be adduced, but

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* See the 43d article. † See the second paragraph in the 18th article.
‡ The experiment has been made, though not mentioned in this paper.
§ See the sixth paragraph of the 24th article.
‖ See the second paragraph of the 46th article.
¶ See the first paragraph of the 7th article.
** See the fourth paragraph of the 42d article.
†† See the 8th article. ‡‡ See the seventh paragraph of the 24th article.
§§ See the third paragraph of the 46th article.

those that have been recited will surely be sufficient to show that the same operations, which will produce these prismatic phenomena, will equally account for those that are formed by the lens; now, as it has been clearly proved, that the critical separation of the colours, which takes place at certain angles of incidence, occasions all the phenomena of the blue and red bows, and of the streaks, rings, and other regular or irregular appearances, that may be seen in a prism, there cannot remain a doubt but that the Newtonian rings observed between object glasses, are owing to the same cause.

53. *Remarks relating to the Newtonian alternate Fits of easy Reflection and easy Transmission.*

In attempting to rescue the science of optics, from what has been so long considered as unsatisfactory for explaining the great question about the cause of the coloured rings, I have made use of a principle, the effects of which have so near a resemblance to those of the suppositious fits of easy reflection and easy transmission, that the author of them might easily be misled by appearances. But although the principle of a critical separation of the colours substituted for these fits, admits the reflection of some rays at the same angles of incidence at which others are transmitted, yet since the Newtonian different refrangibility of light will account for these critical reflections within glass, and the equally critical intromissions from without, we can have no longer any reason to ascribe original fits to the rays of light, which in the first part of this paper, they have already been proved not to possess, and which now, in all prismatic experiments, I have shown are not necessary for explaining appearances that may be accounted for without them.

Fig. 1

E   D

F   ε   δ   γ   β α   G

Fig. 2

F   ε   δ γ β ι   G

Fig. 4.

Fig. 5.

Fig. 7.

Fig. 8.

Fig. 10.

Fig. 11.

g   1 o r

F   g'

I

g'' g' 1'' p' o'' r'' y' o' r'

F   g' g y'
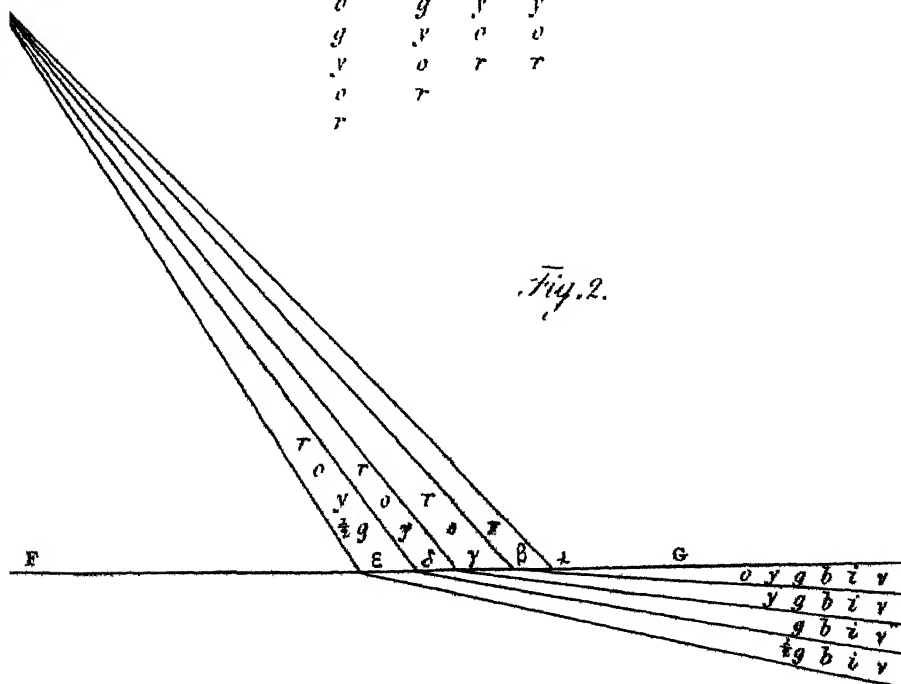
XVIII. *An Account of a Calculus from the Human Bladder of uncommon Magnitude.* *By Sir* James Earle, *F. R. S.*

Read June 15, 1809.

SIR WALTER OGILVIE, Bart. of Dundee, an officer in the regiment of Scotch Greys, at the age of twenty-three, active and healthy, was crossing the ferry at Leith, when he received a blow on his back, from the boom of the vessel, which paralized the pelvis and lower extremities. During two months he was obliged to have his water drawn off; for fourteen months he remained in bed, or in a horizontal posture, and though he then recovered the use of the bladder, and of his limbs, sufficiently to walk across the room with the help of crutches, and also to ride, when placed on an easy low horse, his health continued many years in a weak and precarious state, while the limbs acquired little additional strength or powers.

About twenty years after the accident, perceiving symptoms of stone in the bladder, he was examined by Mr. BENJAMIN BELL at Edinburgh, and a stone was felt, which was judged to have attained a considerable size; the operation of extraction was then recommended, but was postponed from time to time, though his health declined, and the irritation and pains in the bladder gradually increased.

Sir WALTER continued to endure this state of existence twenty-eight years from the time of the accident, when he

became unable to make water in an erect position; this inconvenience increased to such a degree, that latterly he could make none without standing almost on his head, so as to cause the upper part of the bladder to become the lower, and this he was obliged to do frequently, sometimes every ten minutes, as the quantity at each time was less than the measure of a wine glass; and when he used exercise, it was tinged with blood.

The principal remedies which had been prescribed for him were aqua calcis and uva ursi; but he never persisted long in the use of either. At times, when the pain was violent, he had recourse to opium by the mouth and per anum: this, added to a naturally costive habit of body, rendered necessary the frequent assistance of aperients.

At the age of fifty-three, thirty years after the accident, the spasms and fits of pain, from the urgent desire to void urine, became so frequent and violent, and his life so completely miserable, that he was determined to have the stone extracted. I received a letter, requesting my opinion whether a paralytic state of the lower limbs was a prohibition to the operation of lithotomy; on my reply to the contrary, he was put on board a ship and conveyed to the Thames, brought in a boat to Hungerford Stairs, and in an easy carriage to Hanover-street, without suffering any inconvenience of material consequence.

Toward the latter end of July, 1808, I visited him, when he gave me a clear and distinct account of what has been related, and added, that the stone could be evidently felt above the os pubis. At first, I much doubted of the large prominent tumor which I saw in the lower part of the belly, being a stone, but on attempting to pass the sound, it would not enter the bladder,

being stopped by a solid mass; and on further examination, I was thoroughly convinced that there was a stone of sufficient size to fill the bladder.

He said he came to England with a determination to have it extracted, if it were possible, and desired my opinion as to its practicability. On such an extraordinary and important case I declined an immediate answer, but requested a consultation; and Mr. CLINE was appointed to meet me. After mature consideration of every circumstance, we were of opinion that the possibility of extraction must depend on the consistence of the stone; if it proved soft, as is well known to be frequently the case, it might be taken away; but if too hard to be broken, it would be too large to be extracted whole, and must be left.

The operation of extracting it above the os pubis was thoroughly considered, and concluded to be uncertain and dangerous, because the bladder, thickened and exquisitely irritable, could not bear to be further distended with fluid, and the stone, although so large, had not raised it sufficiently high to obviate the danger of wounding the peritoneum, and penetrating into the cavity of the abdomen: the usual lateral operation was therefore judged to be the only safe and probable means to be attempted.

After some days consideration, Sir WALTER, thoroughly and perfectly aware of the difficulties which might reasonably be apprehended in the extraction, from the magnitude, and also from the uncertainty of the structure and consistence of the stone, determined to submit to the operation, and Mr. CLINE was requested to perform it.

On Thursday, August 11th, he was placed in the usual

situation, and the proper ligatures were applied; but it was soon found that the lower limbs were so incapable of action or resistance, that they were left unconfined. The staff could be passed in no farther than the neck of the bladder; the division of the urethra and prostate gland was made with the scalpel and probe-pointed bistouri: when this was accomplished, it was found impossible to introduce any kind of forceps; but on pressing hard with the finger, part of the stone felt soft, gave way, and made some room for the forceps, which brought away several portions, and with the assistance of a scoop, as much stone was extracted as would have filled a large tea-cup; but the great mass, beyond what the finger could reach on either side, still remained hard and impenetrable, and after repeated trials with forceps of different kinds, and of the strongest powers, it was found impossible farther to reduce the size of it, or take it away.

The patient bore the several attempts, and the necessarily protracted operation, with great firmness; probably, from the paralized state of the parts, the natural acuteness of sensation was blunted; however, as from weakness and fatigue he was becoming much exhausted, and the complete extraction appeared clearly out of our power, it was judged right to relinquish any further endeavours.

No hæmorrhage ensued, he became calm and composed, and passed a tolerably good night; the next day he complained only of the same kind of spasms, and frequent pressing desire to void urine that he had been accustomed to feel, and not in any very great degree more acute. As some proof of this, he was frequently inquiring when I thought the wound would be in a state to admit of the extraction of the remainder.

The second night passed nearly the same, without tension of the belly, the urine flowing through the wound, and some by the penis; the third day he complained much of the tenderness of the abdomen, which was tense and painful, and peritonitis seemed rapidly to be taking place, which, however, was lessened and quieted by bleeding and fomentations, and he again became easy: this state continued for several days, but he complained much of the frequent returns of his former spasms, after each of which a small quantity of urine was evacuated. He was nourished with broths, jellies, &c. but would take nothing solid, not even bread in any form; after the fifth day, he ate a few oysters, some fish, or chicken, drank occasionally of porter, and his health and strength were improving; but though these favourable symptoms continued, with the abdomen soft and easy, so as to bear examination by the hand, and all inflammatory action was subsided, yet the repeated spasms continually broke his rest, and kept him in a constant state of irritation, obliging him to violent efforts in resisting them, and to get instantly on his knees, with his head low on the bed, to enable him to expel the urine; and one spasm frequently succeeding before the former had well subsided, kept his whole frame in continual agitation, to the greatest possible degree that nature could bear. All these sensations and occurrences were very similar to what he had experienced for several months before he left Scotland; and it was then the opinion of Mr. STEWART, a medical gentleman who accompanied Sir WALTER to England, that his complaints were increasing to such a degree, that it was scarcely possible for him to exist much longer. In his present situation, of course, such shocks and disturbances of the whole animal economy acted with

increased effect, in proportion as he became less capable of supporting them. Toward the eighth day from the operation, he was visibly growing weaker, his pulse smaller and quicker, his little inclination for food became less, and he was with difficulty prevailed on to take any; some cordial medicines, however, in some degree revived him; but on the ninth day he grew more impatient, feverish, and restless, and on the twenty-first of August, ten days after the operation, he desired not to be teased to take any thing more; when, covering himself completely with the bed-clothes, he quietly resigned a most singularly miserable existence.

Examination after Death.

On opening the abdomen, the bladder was found much diseased and thickened, firmly embracing a stone of extraordinary magnitude, and appearing to be completely filled with it. On dividing the bladder from the os pubis backwards to the rectum, the stony mass was uncovered, which I attempted to take away with the largest forceps; but it was impossible. It was then raised by getting the hand under it, with considerable difficulty, as the cohesion between the bladder and the stone was very strong, though there did not appear to be any diseased or distinct adhesions. When taken out, the form of the stone appeared to have been moulded by the bladder; the lower part, having been confined by the bony pelvis, took the impression of that cavity, and was smaller than the upper part, which having been unrestricted in its growth, except by the soft parts, was larger, and projected so as to lie on the os pubis.

A large excavation had been made in the lower part, which

lay on the neck of the bladder, by the operation. The internal structure was thus exposed, in which appeared distinct stones or nuclei, now consolidated into one mass, disposed in layers.

The weight of the stone was forty-four ounces, or three pounds four ounces (apothecary's weight), the form of it elliptical, the periphery, on the longer axis, sixteen inches, on the shorter fourteen.

The kidneys were altered considerably in their texture, and their pelvis much enlarged, the left was pressed up higher than natural, and adhered firmly to the spleen. The right was attached to the ascending colon, and general adhesion had obtained between all the surrounding parts. The ureters were much increased in their dimensions and thickness, and were capable of containing a considerable quantity of fluid; they were in fact supplemental bladders, the real bladder having become nothing more than a painful and difficult conductor of urine, which trickled down in furrows formed by it on the superior surface of the stone. This clearly explained the cause which obliged the patient, when compelled to evacuate urine, to put himself in that posture which made the upper part of the bladder become the lower, by which means a relaxation or separation was allowed to take place between the bladder and the stone, so that the ureters had an opportunity of discharging their contents; when the body was erect, their mouths, or valvular openings, must of course have been closed, by the pressure of the abdominal viscera on the bladder against the stone.

His difficulty of voiding urine, appears to have gradually increased as the bladder became more and more filled with the stony matter, and the extremity of his distress did not come

on till the whole cavity was nearly choked up. From the appearances on the examination, it is more than probable that the mouths of the ureters would have been soon completely closed, when a total, and consequently fatal suppression of urine must have taken place.

The disease probably originated when the patient was obliged to continue such a length of time on his back, in which position the surface of the water only may be supposed to have been, as it were, decanted, and the bladder, seldom, if ever, completely emptied; thus in a constitution, perhaps naturally inclined to form concretions, the earthy particles subsided, and by attraction soon began to lay the rudiments of a stone, which was not felt above the brim of the pelvis till many years after, but from that time the gradual increase of it was perceptible to the patient, and his medical friends: from this circumstance, as well as the shape of it, the stone evidently appears to have commenced within the pelvis, and in the lowest part of the bladder.

The texture of it appeared different from the generality of calculi, and to contain more animal matter. In a short time it became highly putrid and offensive: after macerating a few hours in fresh water, the saline particles in its composition began to separate, large flakes separated from its surface, and I was convinced, that if the maceration had been continued the whole of the outer part would have crumbled away; it was therefore suffered to dry, and the putrid effluvia gradually went off. As a proof that animal matter abounded, the stone having been protected from the flies, maggots at the end of a fortnight, which were

Dr. Powel examined its chemical composition, and informs me, that it decidedly consists of the triple phosphat of ammonia and magnesia, with phosphat of lime, forming together the fusible calculus of Dr. Wollaston (Phil. Trans. 1797.) mixed with a certain portion of animal matter, which was separated and floated under a membrane-like form, on the solution of the salts in diluted acids. That this composition is demonstrable by all the usual relations to alkalies and diluted acids, and the precipitates from solutions in the latter, by ammonia, and also by its fusion into enamel under the blow-pipe, and that the general external character of the calculus establishes the same fact by its semi-transparent appearance, and regular prismatic crystals of the triple salt, discoverable by a magnifier, between the more compact layers.

The nuclei were examined solely with the blow-pipe, from a wish not to disturb so fine a specimen beyond what had been done by the operation. By this examination they were found less fusible than the general mass, and appeared to contain a larger proportion of phosphat of lime, for by admixture of a portion of triple phosphat of magnesia, taken from another part of the same calculus, they were rendered as fusible as the rest.

This calculus accords entirely with the description given by Fourcroy (Systême de Connoisance Chemique), and confirms his farther observations on this species. " Ce sont aussi les " concretions urinaires les plus volumineuses de toutes ; elles " ont depuis le grosseur d'une œuf jusqu'à une volume qui " occupe toute la vessie, en la distendant même considerable- ' ment." From this passage, it should seem that similar instances had occurred to Monsieur Fourcroy; but from my

The annexed Plates are representations of its size and figures.............

Plate XVI. shews the cavity which was made by the instruments.

It is preserved in the Museum of the Royal College of Sur-geons, London.

own observation, and from all the information I have been able to collect, no calculus from the human bladder, of such magnitude, has been hitherto exhibited, or described, in this country.

JAMES EARLE.

Hanover Square, Dec. 26, 1808.
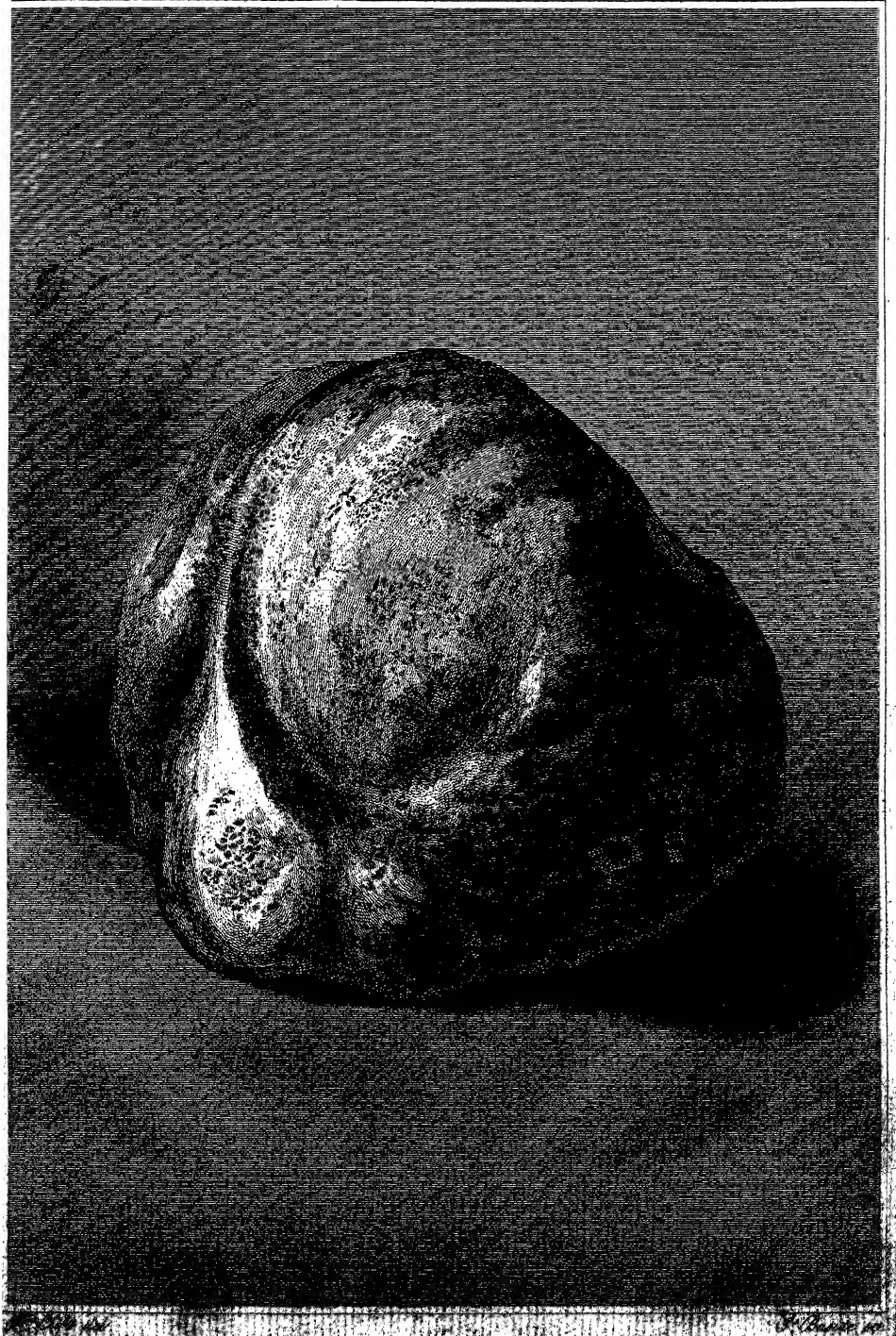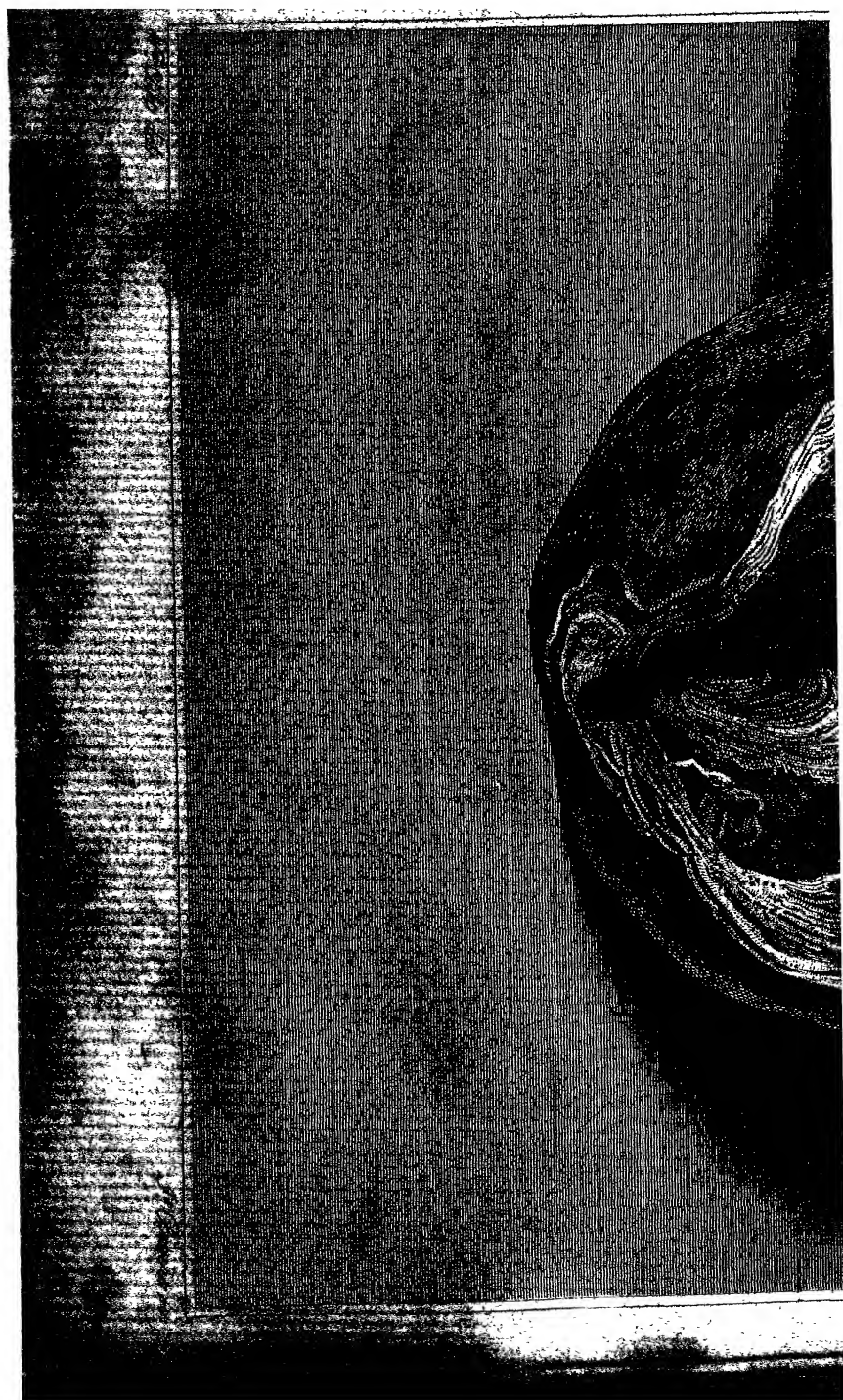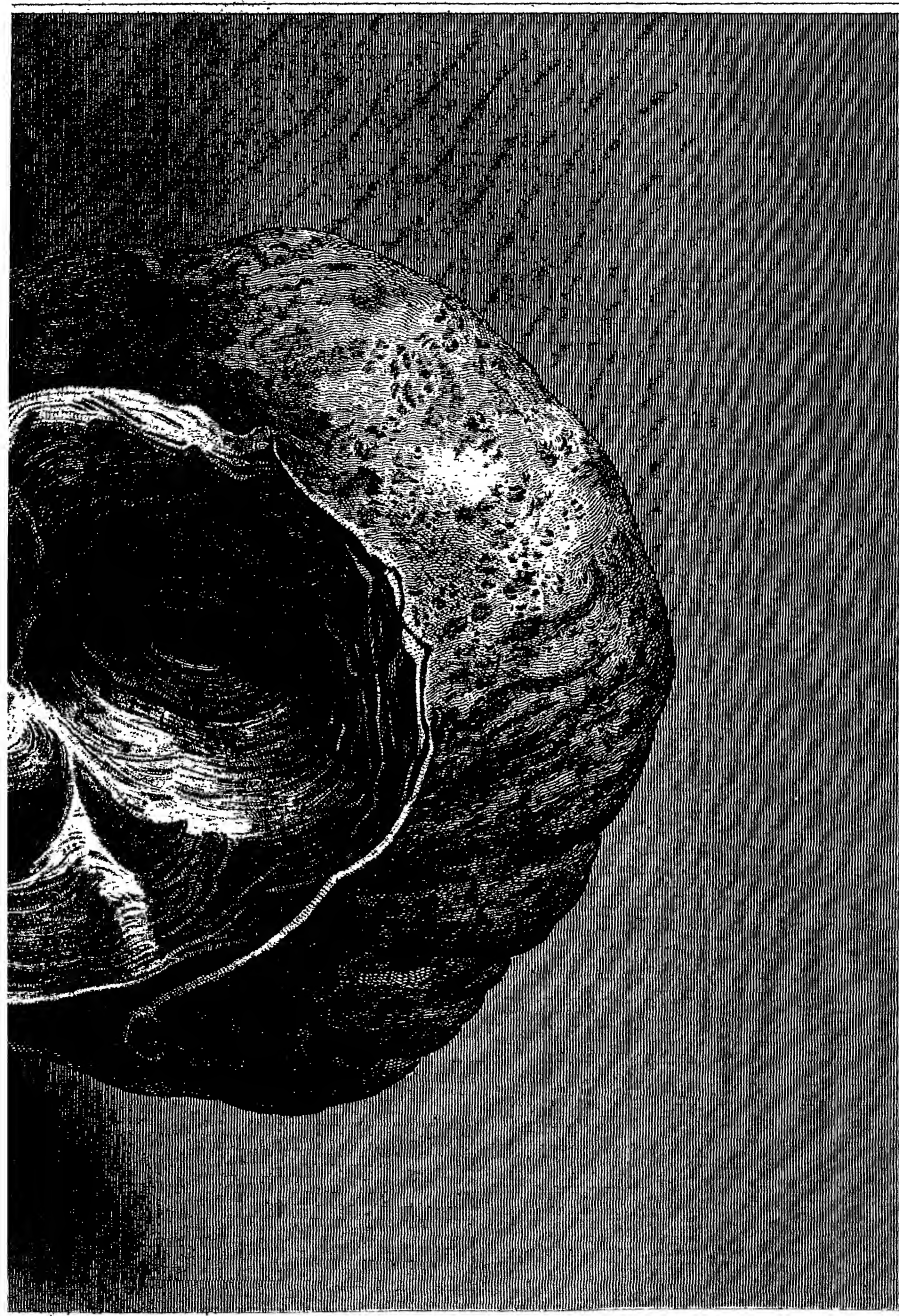
XIX. *On expectorated Matter.* *By* George Pearson, *M. D.*
F. R. S.

Read June 15, 1809.

THE attention of physiologists has been very much with-
drawn, for the last half century, from the consideration of the
different states of the circulating and secreted fluids, in conse-
quence of the opinion that the nervous and fibrous or mus-
cular systems can afford satisfactory interpretations of the
phenomena of living beings; and on account of the disgust
produced by the visionary properties and groundless hypo-
theses, originating in the humoural doctrines of GALEN. But
late experiments have manifested, that various things taken
into the stomach can be made at pleasure to produce consider-
able effects, by impregnating sensibly the blood and urine, as
well as the milk, sweat, and perhaps saliva. Further; the fine
experiments of Professor COLMAN have shewn, that the conta-
gious glanders may be excited in the ass by the transfusion of
the blood of a glandered horse, and the matter from the nose
of the glandered ass can produce this disease in the horse or
the ass.* Hence I apprehend it is reasonable to expect, that
the farther investigation of the properties of the animal
fluids will afford gratifying instruction to the Researcher in

* Mr. COLMAN alleges, that there is not a sufficient quantity of blood, in a single
glandered ass, to excite the glanders by the transfusion of blood into the horse.

natural science, and important practical information to the Physician.

On the present occasion, I desire the honour of communicating the knowledge I may have acquired, by investigating the properties of expectorated matter secreted by the bronchial membrane. The appearances of this substance serve to regulate the judgment of the Physician concerning several diseases of the lungs; but especially of that of pulmonary tubercles which yearly destroys 120,000 to 140,000 subjects of the United Kingdom. It is fit that I remark, that I do not notice in this paper the ingenious experiments of several learned Chemists, because by so doing I should be led into a detail of too great extent for my design.

The numerous varieties of expectorated matter, according to my observation, may be arranged and characterized under the following seven heads:

I. The jelly-like semi-transparent kind of a bluish hue, excreted in the healthy state.

II. The thin mucilage-like transparent matter, so copiously expectorated in bronchial catarrhs.

III. The thick opaque straw coloured, or white and very tenacious matter, coughed up in a great variety of bronchial and pulmonary affections; especially in that of tubercles.

IV. Puriform matter secreted without any division of continuity, or breach of surface of the bronchial membrane, very commonly occurring in pulmonary consumptions.

V. The matter which consists of opaque viscid masses, together with transparent fluid; or the second sort above stated, with nodules of the third or fourth kind.

VI. Pus from the vomicæ of tubercles.

VII. Pus from vomicæ by simple inflammation of the lungs, and without tubercles.

Other kinds of matter are occasionally coughed up, such as calculi,—masses of self-coagulated lymph—serous fluid—blood itself,—and perhaps the vascular substance of the lungs; but I do not write on these matters, because they either do not belong to any particular recognized disease; or they are rare occurrences in some well known disease, and are too obvious to require description.

§ I. *Sensible, or obvious Properties.*

1. *The jelly-like matter,* as already said, is excreted in the best health, as well as sometimes in disease. It is mostly coughed, or hawked up, in a morning soon after a night's repose, during which it seems to accumulate. A few masses, or nodules, then appear of the consistence of jelly, and of the size of a pea to a hazle nut. It is also at any time liable to be excreted, in consequence of various extraneous matters irritating the fauces, to the amount of a few nodules. It is of a grayish colour or inclining to blue, with black specks; and it is rarely whitish in nodules. The consistence is that of jelly, but of much greater tenacity. It has a barely perceivable taste of common salt, or muriate of soda. It commonly floats on water, but by agitation to disengage air bubbles, it sinks. It has no smell. To the naked eye, or assisted by a single magnifier, this matter seldom appears uniform, but consists of a mixture of opaque and transparent masses of irregular figures. With the compound microscope, spherical

particles were perceived, though few in number, when duly diluted. The presence of an alkali I could in no instance perceive, by means of the usual tests, *namely*, turmeric paper, litmus paper slightly reddened by vinegar, and cloth stained with violet juice; nor was an acid denoted by means of litmus paper, except when I had reason to believe it was derived from various acid substances taken with the food, or drink, adhering to the inside of the mouth and fauces.

2. *The mucilage-like expectorated matter*, according to my observation, occurs much less frequently than the other sorts. It appears suddenly in great abundance in certain bronchial catarrhs. I have seen it to the amount of two, or three pints in twenty-four hours. It is also secreted, but less copiously in paroxysms of spasmodic asthma, and of the hooping cough; and but rarely in pneumonic, or pleuritic inflammations, and in some chronical organic diseases of the heart and lungs.

This matter is a transparent uniform fluid of the consistence of white of egg; or of a mucilage compounded of about one part of Arabic gum, and four or five parts of water. It is colourless,—has a fleshy smell—has a brackish taste. After standing eight or ten hours, a deposit takes place of fibrous, leaf-like, or curdy masses, some of which are seen suspended in the clear fluid. In some cases nodules of opaque thick ropy matter, at certain times, accompany this mucilage-like matter. Under the simple magnifier I perceived irregular figured masses partly in motion and partly suspended. With the microscope, globules were seen; but larger considerably than those of the blood, and much less numerous. With the usual tests there were no indications of alkali, nor of acid,

provided the matter was unmixed with other things. It usually floated, or was suspended in water, when first expectorated; but on standing in the water it fell to the bottom, evidently owing to the disengagement of air bubbles.

By standing exposed to the air in warm weather, it sooner grew fœtid than pus of abscesses; without becoming opaque. Neither could I render it opaque or thicker, by exposure to a stream of oxygen gas for an hour; or by exposure of it in a jar of this gas for a month.

3. *The opaque ropy matter* above-mentioned.

1st. It is secreted most copiously in that very common, and extensively epidemial disease of our climate, the *winter-cough*, occasioned by tubercles, to the amount of half a pint to a pint in twenty-four hours; especially during the winter season for several successive years, and sometimes during the whole of a long life, after the age of forty or fifty years. 2dly. It is often the expectorated matter of the pulmonary consumption of young persons, also occasioned by tubercles, but frequently mistaken for the pus of abscesses or vomicæ. 3dly. It appears, oftentimes, in pneumonic or bronchial inflammation with fever, seemingly being a beneficial discharge; as well as in some instances at the close of a fever without concomitant inflammation of the lungs. 4thly. A severe paroxysm of spasmodic asthma is often terminated in the excretion of this kind of matter. 5thly. A secreted substance of this sort is sometimes expectorated in various chronical organic diseases of the lungs, the heart, aorta, and parts contiguous to the lungs, which occasion difficult transmission of blood through them.

In all these instances the matter by expectoration is of the

consistence of thick cream, or of thin toasted cheese ; so tough
as to hang in the form of a rope, four or five inches in length,
on pouring it from one vessel into another. Its aggregation is
such that it is readily detached in large masses from the vitreous
surface of vessels. It is not unusual for small black, or reddish
spots, and streaks, to appear on the surface of this sort of expec-
torated substance. A pretty large bulk of it is seldom through-
out uniform ; but it is frothy, and exhibits opaque masses of
various hues with transparent matter interposed. The *colour* is
yellowish, straw-coloured, and white, or gray : it also, though
seldom, is greenish and bluish. The *taste* asserted by patients,
is, in their own terms, various, *namely*, saltish, nasty, faintish,
sweetish, luscious, or like that of a sweet oyster,—a sharp
or sour taste is the most rare. The only *smell* which I have
perceived is that of flesh, but very frequently there is none.
When any offensive or pungent smell was perceived, imme-
diately after expectoration, I have always found that it was
owing either to the foulness of the vessel in which it was re-
ceived ; or it was from extraneous matters in the mouth, and
from decayed teeth.

This opaque viscid substance, being duly diluted with dis-
tilled water was examined with microscopes of common, as
well as of very great powers : by means of any of them
crowds of spherical particles were seen passing to and fro, in
currents, not unlike those of the blood ; except that they were
larger. These globules I could not destroy, nor alter in form,
by trituration ; nor by long boiling in water ; nor by exsicca-
tion, and again dissolving in water ; nor even by coagulation
with mineral and vegetable acids, with alcohol, with sulphuric
ether, or with tannin, and alum ; nor by mixture with caustic

alkalies in a proportion which leaves the liquor turbid; nor for some time after the putrefactive process had appeared. But these globules disappear with such a proportion of sulphuric acid as detaches charcoal; or of nitric acid, and of liquid potash, as produce a clear solution: also by charring by fire. It is perhaps superfluous to remark, that these atomic globules are quite different from the air bubbles usually entangled in this kind of matter, as perceived by the microscope; the latter differ much from the former, in being of far greater magnitude—in being less numerous—in being transparent, and disappearing on agitation, or heating the matter, or even by mere standing.

For the most part this expectorated substance swims on water; but by agitation or stirring to disengage air bubbles, or by merely standing, it sinks. Some of the lumps suddenly hawked up, immediately fall to the bottom of a vessel of water. No signs of either acid, or alkali, appeared on the trials of this matter with well known reagents, provided it was free from extraneous matter; but it was apt to betray acidity from things taken with the food or drink.

4. *Puriform matter.* I have seen this matter expectorated in several diseases in the quantity of two or three ounces to half a pint in twenty-four hours, on some rare occasions, without any breach of surface. I believe it would be considered by every one to be *pus*, having the properties commonly admitted to be those of this substance. It will however, perhaps, only be just to call it *puriform*, for the present, as it appears to me probable, that I shall hereafter be able to show that it possesses properties not belonging to pus of abscesses, although in the obvious, or sensible properties, it is similar to

such pus. Accordingly this expectorated matter is not only opaque, white, or yellowish, and as thick as the richest cream, but it also has not more tenacity than cream. It is not apt to entangle air, and therefore it immediately mingles with water, rendering it milky; and presently subsides to the bottom, leaving the water clear, or at least whey-coloured. It appears to the naked eye uniform in its texture ; and nearly so under the simple lens : but under the microscope thousands of globules similar to those of the blood are seen, which are indestructible as those above related belonging to another kind of expectorated matter.

The substance, of which I am now speaking, is most frequently excreted in the latter stages of pulmonary phthisis, for many weeks successively. It is taken for granted that this matter is from a breach of surface or ulceration ; but on examination after death, such a state was not found, in many instances, under my observation, although the lungs were as usual full of tubercles and vomicæ. This puriform matter is occasionally expectorated in certain other diseases. The last summer my colleague, Dr. NEVINSON, furnished me with several ounces of this sort of substance, but of a greenish hue, and of the consistence of thin cream ; which was expectorated by a woman in the third week from the attack of the measles. In a few days she died. On examination of the lungs very carefully, by the excellent house surgeon of St. George's hospital, Mr. DAWES, no ulceration could be discovered in the trachea or in the bronchial tubes ; nor were any tubercles, or abscesses found in the lungs. The patient, according to my information, had expectorated more than a pint of this fluid every twenty-four hours for a week before death,

In another hospital case, a man laboured under a cough with spitting of matter, which all who saw it called pus, and as usual it was considered to arise from an ulceration, or suppurated tubercles; but, on examination after death, the disease was ascertained to be condensation of the lungs, to the consistence of liver, with water in the cavities of the chest, and nothing more.

5. *Opaque viscid matter of the third, and perhaps fourth sort,* above distinguished, appearing in nodules, and irregular figured masses, *mixed with transparent slimy matter of the second sort.*

It is not unusual to see the mixture of these two different kinds, from severe fits of coughing in that constant epidemy of the British islands, the winter chronical pneumonia.

Different parts of the bronchial membrane being in different states, may account for the secretion of the two different matters. This seems more probable, than that these different matters should be secreted from the same part; although it is true that the same part does secrete at one period transparent thin slime, and at another an opaque thick matter. The former is occasioned by great irritation of the membrane, and the latter is the effect of a more gradual secretion with much less irritation.

For the sake of brevity, I avoid a further description. The practical application of these observations, however important, would not be suitable in this place.

The sixth and seventh kinds of expectorated substances being secreted after a quite different manner, and being very different in their nature from the preceding five kinds, I shall not give an account of them in this paper.

§ II. *Agency chiefly of Caloric.*

1. No effect of importance is produced by this agent, until the temperature of the expectorated matters is raised to about 150° of FAHRENHEIT's thermometer: then the state of aggregation is evidently altered, the viscidity of each of them being diminished. At about 155°, coagulation begins to be quite evident in the first, third, fourth, and fifth kinds of matter—that is, curdy masses of various magnitudes appear in a milk-like, or whitish liquid. On elevating the temperature to 160° or 170°, a large proportion of curd is formed; but the proportion of the curdy matter to the liquid is very different in different specimens, The viscid texture, or tenacity of the expectorated matters, is by this treatment destroyed. The milky liquid decanted, after standing ten or twelve hours, affords, on evaporation to dryness, about three to four grains of residue from each 100 grains.

This liquid passes very slowly through the paper filter. The filtrated liquor affords scarcely more than one per cent. on evaporation to dryness. By repeatedly boiling in successive portions of water, the whole, as far as I could judge, of a given quantity of the curd might be diffused to form a whitish liquid; which on evaporation to dryness appeared to afford a residue of the same kind (except in containing a smaller proportion of saline substances), as the milky liquid which was separated from the curd on the coagulation of the expectorated matter.

The second kind, called *mucilage-like transparent matter,* does not afford curdy masses at the temperatures abovementioned; but its viscid texture is destroyed, and it becomes a whey-like, or somewhat milky liquid; and, on examination

with a magnifying glass, it appears full of curdy particles.—
After this agency of caloric, the expectorated matter is much
less prone to putrefaction.

2. Distillation of the expectorated matters to dryness af-
forded a perfectly limpid water, which had a peculiar smell,
but no impregnation with ammonia; nor with any other sub-
stance which could be detected, except a little carbonic acid.

The residuary matter in a brittle state of dryness, afford-
ed by evaporation, varied between two and a half and ten
per cent. of the expectorated substances. The second kind
yielded one thirty-fifth to one forty-fifth of its weight of
brittle residue. The first kind afforded one twentieth to
one twenty-fifth of residue. The third kind afforded very
different proportions of solid residue, according to its consis-
tence, *viz.* one tenth to one eighteenth of its weight. The
fourth kind gave one twelfth to one fourteenth of brittle
matter. The fifth kind yielded very different proportions of
residue, according to the very different proportions of trans-
parent and opaque matter, of which it consisted—it varied
between one eighteenth and one thirtieth.

3. All these exsiccated substances on exposure to air, grew
more or less moist, or at least were no longer brittle; but be-
came somewhat soft, and, proportionately to the state of mois-
ture, were augmented in weight. The thinner the expectorated
matters, the moister and the greater increase of weight they
generally experienced. But parcels of the same consistence
from different patients sometimes differed much in degree of
moisture, on exposure to the air. I have found some parcels of
the second and fifth sorts of expectorated substances grow quite
moist, and receive an increase in weight of three per cent. If the

residues were kept in close vessels, they remained in a brittle state. Larger parcels of exsiccated matter become more moist than smaller ones of the same kind in the same circumstances.

4. The milky and curdy liquids, which separated from the curdy masses (1) being poured off; and also the curdy masses being by pressure rendered dry; the liquids were evaporated to dryness, but became moist on exposure to the air. The curdy masses were by evaporation rendered brittle, and remained so in the air. The residues of the evaporated liquids were said to taste extremely salt, and the exsiccated curdy matter was tasteless.

5. The milky liquids (4) concentrated by evaporation, did not indicate any disengaged acid, nor alkali to the usual re-agents —By triturating these liquids with lime, a little ammonia was discharged—by trituration with concentrated sulphuric acid, the muriatic acid was disengaged—with phosphoric acid, and also with tartaric acid on trituration and heating, a pungent smell was perceived, somewhat like that of the acetous acid.—On burning to a brown ash the saline residue afforded by evaporation of these liquids, the predominating taste of it was that of muriate of soda. This ash readily melted,— being moistened, it turned turmeric paper to a reddish brown colour, and changed turnsole paper, reddened by acetous acid, to a deep blue—on exposure to the air, it partially deliquesced—the dissolution, by boiling in distilled water, afforded supertartrate of potash on the addition of the tartaric acid; and a red precipitate was occasioned by nitro-muriate of platina* This incinerated and fused saline residue by other

* The knowledge of this re-agent, I believe, the chemical world owes to Dr. Wollaston.

'trials, was proved to contain phosphoric acid, and lime; with traces of sulphuric acid, magnesia, iron, and perhaps silica; but the chief ingredients were muriate of soda, and potash.

6. The curdy matter after expression (4) afforded a much smaller proportion of brown ash than the fusible saline residue (5). It required an intense fire for fusion in a platina crucible. The fused mass did not deliquesce, but it grew somewhat moist on exposure to the air. It contained a much smaller proportion of potash than the former fused matter (5); also much less of muriate of soda, but a far larger proportion of lime and phosphoric acid with traces of sulphuric acid, magnesia, oxide of iron, and perhaps silica.

7. (*a*) 15400 grains of the third sort of expectorated matter on exsiccation, afforded 960 grains, that is, one sixteenth of brittle substance, or about six per cent., and of course this kind of matter contained about ninety-four per cent. of water (§ II. 2). This dried matter was reduced to a charred state by exposing it to fire in a WEDGWOOD white crucible. In this process it inflamed, emitted the usual smell of burning animal matter, especially of bone, and swelled prodigiously; at the same time a black oil was compounded rendering the mass soft during the inflammation. I could not distinguish the smell of sulphur, but there was, in one part of the burning, a smell, to my sense, of phosphorus.

(*b*). This charred matter was kept in a state of ignition in a platina crucible, till it no longer remained in a powdery form, but was reduced to a comparatively small bulk of a substance of the consistence of paste in an intense fire. By continuing the fire, the charge at length was fused; and after being kept in a state of fusion to be quite fluid for ten minutes,

the fire being withdrawn, a white, brittle, apparently saline matter, like melted common salt, was easily detached from the platina crucible, which in some places had received a red tinge.

(*c*). The melted matter (*b*) weighed fifty-nine grains: of course, this saline residue amounted to $\frac{1}{261}$ of the expectorated matter, and to one sixteenth of this expectorated matter exsiccated. It tasted only of muriate of soda—it had no smell —it effervesced with acids—it betrayed the presence of alkali to the tests above-mentioned—after a few days exposure to the air, it partially deliquesced—it precipitated supertartrate of potash with tartaric acid, and emitted no ammonia with lime ; nor sulphur with muriatic acid discoverable by the most delicate tests.

(*d*). The fused matter (*c*) was boiled with three times its weight of distilled water, in which about one half appeared to dissolve. The clear liquid decanted from the sediment and evaporated, yielded crystals of muriate of soda with a much smaller quantity of spicula, or needle-shaped crystals; and saline matter which appeared under a lens not definitely crystallized. A second boiling of the sediment, with twice its quantity of water, afforded almost entirely muriate of soda. A third boiling gave a few crystals of this salt only, as appeared under the magnifier. A fourth boiling, in an equal weight of water, afforded no saline matter.

(*e*). The saline matters (*d*) amounted to forty-five grains when evaporated to dryness. I collected by means of a tooth-pick from amongst the cubical crystals, as much as I could of the spicula and uncrystallized saline matter. These parts effervesced and precipitated supertartrate of potash with tartaric

acid, and certainly afforded no soda-tartrate of potash—they also afforded a precipitate with nitro-muriate of platina—being saturated with acetous acid there was still a slight precipitation with muriate of baryt; for without acetous acid, there was a most copious precipitation with this re-agent, but the greater part of the precipitate was dissolved by acetous acid, added so as not to supersaturate it.—Oxalate of ammonia did not occasion a precipitation,—with nitrate of silver an abundant one took place—lime water produced only slight turbidity. The muriate of soda amounted, in this saline mass of forty-five grains, to thirty-five grains, or nearly to one grain in 450 of expectorated matter; the rest was subcarbonate of potash amounting to one grain in about 1540 grains of expectorated matter, with which was mixed a minute proportion, probably, of sulphate and of phosphate of potash.

(f). The undissolved matter (d) boiled with muriatic acid gave a turbid liquid, but on standing, nearly the whole appeared to have been dissolved; a small proportion of sediment only took place in a transparent liquid, which was boiled till it no longer parted with muriatic acid.—This dissolution being exsiccated grew liquid on exposure to air; and oxalate of ammonia gradually added, produced, as I decidedly ascertained, the precipitate of oxalate of lime.

(g). The filtrated residuary liquid (f); with muriate of baryt gave immediately a copious precipitation—with lime water there was milkiness produced, and subsequently a white precipitation which did not disappear on adding a small proportion of acetous acid—prussiate of potash occasioned a greenish blue colour without precipitation—succinate of am-

monia produced a milky liquid—no effect was observed from tartaric acid.—There being a precipitation with caustic or pure ammonia, as well as with potash, and with the carbonates of the alkalies, it was supposed magnesia was present: and the dissolution of this precipitate in muriatic acid, and in acetous acid, gave no precipitate with oxalic acid. Some of the muriatic dissolution, previously to precipitation with oxalate of ammonia (f), being evaporated to dryness, the residue was ignited; but if magnesia was present, as well as lime, it was in too small quantity to be distinguishable from the lime, by composing sulphate. The precipitate now under examination, was certainly not mere magnesia, for it melted into an opaque globule under the blow-pipe—it was not phosphate of lime, for with sulphuric acid, a somewhat bitter and sour substance was compounded, which afforded a precipitate with ammonia, but none with oxalate of ammonia. It was a phosphate not only on account of its fusibility, but because a curdy appearance was occasioned by the mixture just mentioned, with sulphuric acid, on adding it to lime water. Neither was it soluble, like phosphate of lime, in phosphoric acid. The quantity of this precipitate was too minute for decisive experiments, but from those related, it seems probable that it was phosphate of magnesia, which was dissolved, as will appear presently, in phosphoric acid, and precipitated by ammonia.

(h). The residuary liquid (g), after the precipitation by oxalate of ammonia, being evaporated to dryness, was easily ascertained to be phosphate of ammonia, with indications of a minute proportion of sulphate.

(i). It remains only to notice the indissoluble matter in

muriatic acid (*f*). I found it to grow soft, and the parts to cohere under the blow-pipe, and with a little potash it readily melted into an opaque globule.

8. To obtain a more satisfactory proof of the presence of sulphur, forty grains of charred expectorated matter were kept in a state of ignition in a platina crucible, with another inverted over it to completely exclude the escape of gas, for two hours. After cooling, the smell of sulphuretted hydrogen gas very evident, on the addition of diluted muriatic acid, and even of water only. Silver was tarnished, and paper wetted with liquid acetite of lead was blackened by this gas. In some of the experiments, while the charcoal was burning off from the charred expectorated matter, I perceived the smell of sulphur, and perhaps of phosphorus.

§ III. *Agency of Alcohol of Wine.*

1. (*a*) 2500 grains of desiccated expectorated matter of the fifth sort, § I. 5 being the one twentieth of 50,000 grains of matter previously to evaporation to dryness, were digested in four pints of alcohol of spirit of wine, of the specific gravity of 815. water being 1000. The mixture was exposed at the temperature of 58° to 68° for a month, during which it was frequently shaken. A tincture, of the colour of red port wine, was then decanted from off a blackish sediment. By means of a press, two ounces more of the tincture were obtained.

(*b*). The undissolved residuary matter being exsiccated weighed 130 grains less than before digestion. On exposure to the air, it remained dry, but it became more flexible. It no longer emitted ammonia on trituration with lime.

(*c*). The tincture thus obtained was distilled readily till there remained about five ounces measure in the retort, and what remained seemed to be chiefly water instead of spirit, with such a quantity of matter dissolved in it, as not to afford liquid by distillation, without frequently spirting into the receiver. The residuary liquid was therefore evaporated to the consistence of a soft resin-like extract of a black colour; which had a salt with bitter taste.

The distilled liquid had a peculiar pungent smell, but not that of ammonia, and it neither reddened turnsole paper, nor rendered violet cloth green.

(*d*). The resin-like extract (*c'*) weighed 140 grains. It was semi-transparent—dissoluble in water, but not coagulable in boiling water—it grew softer on exposure to air—it was uncrystallizable—it betrayed no signs of alkalescency nor of acidity, except just giving turnsole paper a reddish hue—under the blow-pipe it burnt like matter from animals, and afforded a fused globule, which indicated muriate of soda, and a large proportion of potash, deliquescing very speedily—with lime, it emitted the smell of ammonia—with phosphoric, and also with tartaric acid, on being heated, an acid smell was perceived which I at first mistook for acetous acid, for I soon found that no such acid was present, not being able to detect a trace of any acid in the distilled liquid from these mixtures —on the addition of acetite of lead, a very copious precipitation of fawn-coloured sediment instantly took place, with the smell most distinctly of apples. The decanted liquid of this mixture was found to be chiefly acetite of potash. On dropping some sulphuric acid upon the fawn-coloured sediment, it constantly emitted the smell of apples. I could not,

however, satisfy myself, that the small quantity of liquid decanted from off this sediment contained a kind of vegetable acid for the first time apprehended in the fluids of animals; because, first, the quantity of product I possessed was so diminished by many experiments, that I was unable to make what I considered to be decisive trials. Secondly, because in subsequent processes I failed in producing the same apple-smelling liquid. Hence I considered that the supposed acid, which had some of the properties of the malic, only occurred occasionally, or that I had been deceived, and that I had procured nothing more than a little of the acid employed for the decomposition, disguised by mixture with the subject of the experiments. The fawn-coloured precipitate was, no doubt, chiefly muriate of lead. Still the experiments fully demonstrate the presence of potash neutralized, either by an acid destructible by fire and dissoluble in alcohol, but hitherto not disunited from animal oxide, or that an oxide of animal matter alone neutralizes the potash, as will be manifested by the evidence of experiments to be related.

(e). Forty-five grains of the residue (c) which had been dissolved in alcohol, being burned in a platina crucible, yielded chiefly potash, and half its quantity of muriate of soda.

(f). Twenty-five grains of the residue (c) were boiled with successive portions of nitric acid, till the oxide of animal matter was decompounded and carried off in the state of gases; and then deflagration took place, leaving subcarbonate of potash with muriate of soda and charcoal.

According to a computation, the 140 grains of resin-like extract (c, d) consisted of twenty-eight grains of potash, and

eighteen grains of muriate of soda, with an inappreciable quantity of ammonia, and perhaps phosphoric acid, besides the oxide of animal matter, and possibly an acid of an unknown kind.

(*f*). The undissolved matter (*b*) was burned in a platina crucible. It afforded a residue, which I could not render fluid by fire, but only of the consistence of paste. On cooling, it was a brittle gray mass weighing fifty-six grains, somewhat salt and gritty to the taste. It consisted of muriate of soda and phosphate of lime, about twenty-three grains of each,— of potash four grains—of fused matter, which by long boiling in muriatic acid yielded phosphate of lime, muriate of lime, and utterly indissoluble vitrefied matter with traces of magnesia, oxide of iron, and a sulphate.

2. Four thousand grains of expectorated matter of the third kind, page 317, § II. 3 were added to two pints of rectified spirit of wine. By agitation, the spirit became at first milky, but presently it grew clear; little curdy masses appearing, which fell to the bottom as a sediment, being in bulk about one fourth of that of the added expectorated matter.

After a month's digestion, the filtrated liquid, on evaporation, afforded a dry extract-like residue, weighing sixty grains. It grew moist by exposure to the air, but not when kept in close vessels. It consisted of the same ingredients, but in very different proportions, as the residue from distilling and evaporating the tincture, page 323, § III. 2, the present residue containing a much larger proportion of muriate of soda, and oxide of animal matter.

Successive digestions of the same matter afforded less and less saline residue, but nearly the same proportion of oxide

of animal matter for three times, but then no saline matter was afforded, but merely animal matter. The residues of the evaporated tinctures of the subsequent digestions did not, like the first, grow moist, but only softer; and the oxide of animal matter from each of them was no longer coagulable, although afforded by dissolution of coagulated matter. It appeared that the animal oxide was of one kind only, and that the whole of it might be dissolved in alcohol, and thereby become uncoagulable, and more easily dissoluble in every kind of menstruum.

3. If a large proportion, namely, two parts of expectorated matter be mixed with two parts of rectified spirit of wine, the matter is in great part, at least, coagulated, but the spirit is rendered milky. The same is true with regard to other menstrua. The reason is obvious. The coagulation is produced by the separation of water from the animal oxide of the expectorated matter, by the attraction of the alcohol, or of acetous acid for the water; but if there is not a due proportion of spirit or acid, the oxide of animal matter retains so much of the water, as to render the liquid milky. A person accustomed to these experiments may determine pretty exactly by means of them, the proportion of water in the expectorated matter, it being directly as the quantity of spirit or acid requisite to produce entire coagulation in a clear liquid; and the proportion of coagulable animal oxide is, within certain limits, inversely as the quantity of spirit requisite for coagulation.

4. Sulphuric ether, being in many properties analogous to alcohol of wine, I digested three hundred grains of exsiccated matter of the third kind, page 317, in four ounces measure of

this menstruum for a month, in a warm room, during which the vessel was often agitated. Three ounces of a black tincture were thus procured, which, on distillation to dryness, afforded sixty-five grains of soft extract. This extract became a little moist on exposure to the air, and was then a little viscid. It burnt with flame like oil to the state of charcoal; and this again on burning, only left two grains of residue, which consisted of muriate of soda, with indications of alkali, and phosphate of lime.

The undissolved residue also remained soft, and could not be made brittle by evaporation. After inflammation and incineration, the usual products were obtained as from matter which had not been digested. This menstruum had therefore dissolved abundantly the oxide of animal matter, and but a small proportion of the saline and earthy parts.

4. Apparently uniform expectorated matter is not of the same consistence through the whole mass; for a few drops of the opaque kind being shaken in half a pint of rectified spirit of wine, the whole does not dissolve, but it is broken into small curdy particles, which fall as a sediment in a clear liquid, seemingly about one-fourth of the original bulk of the matter.

§ IV. *With Water.*

1. None of the kinds of expectorated matter are readily diffusible through cold water, except the second and fourth, 316 and 319; and by agitating them some fibrous pieces detached; also on inspecting the water after this appears full of small masses, or motes. On standing, these suspended masses become a sediment; which is the

·case, although the proportion of expectorated matter be exceedingly small to that of the water.

2. When very hot water is used, namely, that of the temperature of 190° to 210°, a still greater number of motes are perceivable, especially with a lens, and the water is rendered milky.

3. Brisk agitation is required, for a due length of time, to diffuse the other kinds of expectorated matter through cold water; but a great number of fibrous and membranous pieces appear, whose form cannot be destroyed or only partially, by shaking, in almost any proportion of water. Three drops of ropy and opaque matter were shaken in half a pint of distilled water. About one half of them was diffused; the rest was in the form of small fibrous, leafy, and irregular figured motes; which, on repose, formed a sediment, and remained in that state three months; although in that time the water became highly fœtid, and sometimes in this experiment the sides of the vessel were tinged black.

4. Agitation of these sorts of expectorated matter (3), in a large proportion of water at the temperature of 170° and upwards, produced a greater degree of milkiness, and a greater number of small masses, which could not be dissolved by long shaking. Putrefaction did not take place so soon in these mixtures, as in those with cold water.

5. If the proportion of the last mentioned kinds of expectorated matter be two or three parts to one of cold water, or under the temperature of coagulation, an uniform mixture may be produced by violent agitation, the water being entangled by the viscidity of the matter rather than chemically united.

6. On boiling the mixtures (5), a great part of the expec-. torated matter is separated in a curdy form, from a milky liquid.

7. If less than two grains of expectorated matter were diffused through five hundred grains of water, no evident precipitation was occasioned by tannin; while with one grain of isinglass jelly, or white of egg, or of serum of blood dissolved in five hundred grains of water, there was an evident precipitation with this re-agent.

8. I could arrive at no useful conclusions, for the distinction of expectorated matter from other coagulable, or from any gelatinous substances by comparative trials with muriate of tin, nitro-muriate of gold, oxymuriate of mercury, acetite of ceruss, and acetite of litharge.

§ V. *Agency of acetous Acid.*

1. Twenty ounces of ropy opaque matter, by being shaken with ten pints of distilled vinegar, were so broken into a fibrous or even vascular form as to exhibit an organized appearance, the bulk being reduced to at least one third of the ropy matter. By repeated agitation and long digestion, the coagulated masses were broken into smaller pieces, but did not appear to be further contracted in bulk, or to dissolve. With some parcels of matter, the vinegar preserved its transparency, with others it became whey-like, the matters being deposited in a curdy state. The mucilage-like expectorated matter, or this mixed with the other kinds, afforded whey-like, or more or less turbid liquids with vinegar.

2. (a.) This second liquid, and the liquid obtained by pres-

sure of the sediments of the last mixture (1), being distilled to about one eighth, the remainder was evaporated to the consistence of a thick extract. The distilled liquid did not appear to have received any impregnation, except what had altered a little the odour. This extract-like residue amounted to one forty-fifth to one eightieth the weight of the expectorated matter, according to the kind of this substance. It varied also according to the proportion of the matter to the acid menstruum.

(*b*). The residue (2, *a*) just mentioned, after digestion a second time, in the same quantity of acid, afforded a smaller quantity of extract-like matter than before.

(*c*). The third digestion afforded still less of this substance.

(*d*). The fourth and fifth digestion gave somewhat less than the immediately preceding one.

(*e*). The sixth digestion yielded nearly the same proportion of extract-like matter as the fourth and fifth.

3. The undissolved matter, after these repeated digestions in vinegar (1, 2), being exposed to fire in a platina crucible, first flamed and partially melted; then became apparently charcoal, which burned away to the state of a brown earth-like substance scarcely $\frac{1}{180}$ of the weight of the substance subjected to fire, and not above $\frac{1}{1600}$ of the expectorated matter by which it was afforded. It consisted chiefly of phosphate of lime, with indications of carbonate of lime, of a sulphate, of a muriate, of silica, or at least vitrified matter, and of oxide of iron.

4. The extract-like matter, from the first digestion of the expectorated matter (2 *a*), by exposure to the air, in a few

days partially deliquesced, affording no signs of alkalescency, but having a peculiar salt taste.

(*a*). A little of this deliquescing part being boiled to dryness, with a large proportion of nitrous acid, on beginning to be ignited, it deflagrated, leaving a blackish saline residue; which soon deliquesced, and being lixiviated, it precipitated supertartrate of potash with tartaric acid, and gave a reddish precipitate with nitro-muriate of platina. The residue also contained lime, for the dissolution in acetous acid afforded oxalate of lime, on the addition of oxalate of ammonia.

(*b*). This extract-like matter (*2 a*), by digestion in rectified spirit of wine, gave a blackish tincture, which being decanted and evaporated, left a residue. This became quite liquid after twenty-four hours exposure to the air. It consisted chiefly of acetite of potash, with an inappreciable portion of muriate of soda, and ammonia neutralized, probably, by phosphoric acid; besides uncoagulable and ungelatinizable oxide of animal substance.

(*c*). The undissolved matter by spirit of wine, just spoken of (*b*), after expression, being desiccated, it remained in a solid state after exposure to the air, only growing a little soft in four weeks time. By combustion, it afforded a difficultly fusible ash, which after fusion was found to consist chiefly of phosphate of lime, muriate of soda, with a little potash; a sulphate, traces of iron, and vitrified matter, which probably contained silica united to the other substances manifested in this fused mass.

extract-like matter, by acetous acid on the second digestion grew soft, but did not deliquesce on exposure to the air

by the first digestion in the same menstruum, in containing a much smaller proportion of potash and muriate of soda, as well as of neutralized ammonia.

6. The extract-like matter, from the third digestion in vinegar (2 c), differed from the former, in containing a still much less quantity of the salts just mentioned.

7. The fourth and subsequent digestions (2, d, e) afforded extract-like substances, which contained scarcely any thing but a very small proportion of earthy phosphates, and indissoluble vitrified matter, produced by incineration and fusion. It did not appear that the oxide of animal matter, dissolved by the distilled vinegar in all the preceding digestions successively, was of different kinds; but it appeared, that its coagulable property was destroyed by dissolution in this menstruum. Accordingly, there is no reason to believe that the whole of this oxide is not dissoluble in the acid here employed, although the requisite proportion may decrease after each digestion, within certain limits.

8. A few drops of opaque ropy matter being agitated in half a pint of vinegar, a number of fibrous masses appear, apparently one fourth or one fifth the bulk of the matter added; and these fibrous forms subsist, notwithstanding continued agitation, totally disappearing only in consequence of long digestion in successive large quantities of this acid.

§ VI. *Some Experiments with different Objects.*

1. To produce a synthetic proof that potash may be neutralized by oxide of animal matter, I triturated ten grains of the exsiccated and coagulated part of expectorated matter freed from all saline substance, with pure potash gradually added,

water, that is, between five and six per cent. of the expecto-
rated matter.

3. The impregnating substances have been shown to be
Muriate of soda, varying commonly between one and a half
to two and a half per 1000 of the expectorated matter—Potash
varying between one half and three fourths of a part per 1000—
Phosphate of lime about half a part of 1000—Ammonia, united
probably to the phosphoric acid ; Phosphate, perhaps of mag-
nesia ; Carbonate of lime ; a Sulphate ; vitrifiable matter, or
perhaps silica ; and oxide of Iron. But the whole of these last
six substances scarcely amounting to one part in 1000 of the
expectorated matter, it would be useless to estimate the pro-
portion of each of them. It is very probable that the propor-
tions and quantities of these ingredients, vary much more than
now represented in different states of disease and health.* It
is very probable also, that some of the ingredients may occa-
sionally be absent, and others of a different kind be present,
agreeably to the different states, on different occasions of the
other secretions.

4. It is manifest that the different states of consistence of
expectorated matter, are owing to the proportion of albumi-
nous or coagulable oxide, but I purposely avoid giving an
account of the different conditions of health, on which the dif-
ferences of consistence depend.

5. The thicker the matter, the smaller I commonly found
the quantity of saline impregnation. Hence, in sudden and
copious secretions of the bronchial membrane, the matter is
assumed to be salt, and to feel hot. In such instances, the
proportion of coagulable matter was small, but that of the

* In one case, the opaque expectorated matter in a pulmonary consumption having
been exsiccated to brittleness, became almost liquid after a night's exposure to the air.

saline impregnations, particularly of the muriate of soda, and neutralized potash so great, that the exsiccated expectorated substance tasted very salt, and presently grew moist, or even partially deliquesced; but the opaque ropy or puriform matter afforded a much larger proportion of exsiccated.residue, which was but slightly salt, and generally only became soft on exposure to the air. This property of growing moist depends upon the potash.

6. Each of the human fluids, according to my experiments, contain neutralized potash; at least, this is the fact of the blood, dropsy fluid, pus of abscesses, and pus secreted without breach of surface; the fluid effused by vesicating with cantharides; the urine; and in course in the very abundant secretion from the nose by a catarrh. The alkali being united to oxide of animal matter in these fluids, it is easily demonstrable.

7. Although I think I have discovered many properties by which expectorated secretion may be distinguished from expectorated pus, I shall not speak of them, on this occasion, further than just to observe that the saline impregnation of pus, particularly that of potash, and muriate of soda is in very much less proportion than in expectorated secretion; and hence it does not become moist after exsiccation, on exposure to the air.

8. It has been, I believe, uniformly asserted, that the circulating and secreted fluids are impregnated with soda; that it is especially in the matter secreted by the bronchial membrane. The experiments of others must confirm or disprove mine. It seems, however, much more reasonable, that the human fluids should be found to contain potash than soda, united to some oxide or destructible acid; because the former alkali is daily introduced with the vegetable food, and with the

drink of fermented liquor; and it is as little likely to be destroyed, as the muriate of soda also induced in the very same way. But our food and drink do not, commonly at least, contain the soda united to a destruc.ible acid, or an oxide.

9. It is plain, from the preceding experiments, that expectorated matter belongs to the class of coagulable fluids, and not of gelatinizable, or, as commonly asserted, mucous fluids. It differs from the coagulable fluid, scrum of blood, in forming a much thicker fluid with a much larger proportion of water: for serum and also the water of blisters, is quite liquid, although they afford, on exsiccation, one twelfth to one eleventh of their weight of brittle residue, while some kinds of expectorated matter, of the consistence of mucilage, afford only one fortieth of dry residue, and others of the consistence of thin paste, afford only one fourteenth of residue.

10. But for the unavoidable extent of this paper, I should trouble the learned Society with various other conclusions and remarks, especially concerning the *globularity* of expectorated matter, which seems to indicate organization. Although ANTONIUS VAN LEWENHOECK, above a century ago, discovered the globularity of the blood, and even noticed it in other animal fluids, neither he, nor any other person, as far as I know, investigated the subject in any fluid but the blood, till by Mr. HOME's acuteness and industry, at a very early period of life, it was observed in pus. I have in this paper related, that expectorated matter, especially the opaque ropy kind, as well as the puriform, is full of globules, and that, except by such agents as destroy charcoal, they are scarcely destructible. Do these spherical particles consist chiefly of organized carbonaceous matter?

XX. *On the Attractions of homogeneous Ellipsoids.* *By* James Ivory, *A. M.* *Communicated by* Henry Brougham, *Esq.* *F. R. S.*

Read June 15, 1809.

1. THE theory of the figures of the planets involves in it two distinct researches. In the first of these, it is required to determine the force with which a body, of a given figure and density, would attract a particle of matter, occupying any proposed situation: in the second, the subject of investigation is the figure itself, which a mass of matter, wholly or partly fluid, would assume, by the joint effect of the mutual attraction of its particles, and a centrifugal force arising from a rotatory motion about an axis. To render the second of these inquiries more exactly conformable to what actually takes place in nature, the influence of the attractions of the several bodies, that compose the planetary system, ought to be superadded to the forces already mentioned.

It is the first of these two researches, of which we propose to treat at present; and we shall even confine our attention to homogeneous bodies, bounded by finite surfaces of the second order.

The theory of the attractions of spherical bodies is delivered by Sir ISAAC NEWTON in the first book of the Principia.* In the same place the illustrious author lays down a method for

* Sect. 12.

determining the attractions of round bodies (or such as are generated by the revolving of a curve about a right line which remains fixed) when the attracted point is situated in the common axis of the circular sections :* and he employs this method to compute the attractive force of a spheroid of revolution on a point placed in the axis.† MACLAURIN was the first who determined the attractions of such a spheroid generally, for any point placed in the surface, or within the solid. The method of investigation, invented by that excellent geometer, is synthetical, but original, simple, and elegant, and has always been admired by mathematicians. When the attracted point is placed without the solid, the difficulty of solving the problem is greatly increased ; and it was reserved for LE GENDRE to complete the theory of attractions of spheroids of revolution, by extending to all points, whether without or within the solid, what had before been investigated for the latter case only.‡ LA PLACE took a more enlarged view of the problem ; he extended his researches to all elliptical spheroids, or such solids whose three principal sections are all ellipses ; and he obtained conclusions with regard to them, similar to what MACLAURIN and LE GENDRE had before demonstrated of spheroids of revolution. In this more general view of the problem, the investigation is particularly difficult, when the attracted point is placed without the solid. The method of investigation, which LA PLACE has employed for surmounting the difficulties of this last case, although it is entitled to every praise for its ingenuity, and the mathematical skill which it displays, is certainly neither so simple nor so direct, as to

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* Sect. 13. Prop. 91.   † Prop. 91, Car. 2.
‡ Acad. des Sciences de Paris, Savans Etrangers, Tom. X.

leave no room for perfecting the theory of the attractions of ellipsoids in both these respects. It consists in shewing that the expressions for the attractions of an ellipsoid, on any external point, may be resolved into two factors; of which, one is the mass of the ellipsoid, and the other involves only the excentricities of the solid and the co-ordinates of the attracted point: whence it follows, that two ellipsoids, which have the same excentricities, and their principal sections in the same planes, will attract the same external point with forces proportional to the masses of the solids. This theorem includes the extreme case, when the surface of one of the solids passes through the attracted point: and by this means the attraction of an ellipsoid, upon a point placed without it, is made to depend upon the attraction which another ellipsoid, having the same excentricities as the former, exerts upon a point placed in the surface.* Le Gendre has given a direct demonstration of the theorem of La Place, by integrating the fluxional expressions of the attractive forces; a work of no small difficulty, and which is not accomplished without complicated calculations.† In the *Mecanique Celeste*, the subject of attractions of ellipsoids is treated by La Place after the method first given by himself in the Memoirs of the Academy of Sciences,‡ founded on the theory of series and partial fluxions. It was in the study of La Place's work, that the method I am about to deliver, was suggested; and it will not be altogether unworthy of the notice of the Royal Society, if it contribute to simplify a branch of physical astronomy of great difficulty, and which has so much engaged the attention of the most eminent mathematicians.

* *Acad. des Sciences de Paris pour* 1783.   † *Ibid.* 1788.   ‡ *For* 1783.

2. Let a, b, c, be three co-ordinates, that determine the position of a point attracted by a solid: and let dM denote a molecule, or element of the mass of the solid, whose position is fixed by the co-ordinates x, y, z, respectively parallel to a, b, c: then, supposing the invariable density to be denoted by unity, if we put $f = \left\{ (a-x)^2 + (b-y)^2 + (c-z)^2 \right\}^{\frac{1}{2}}$ the distance of the molecule from the attracted point, the direct attraction of the molecule on the point will be $= \frac{dM}{f^2}$. This force of attraction is next to be decomposed into other forces, having fixed directions independent on the position of the attracting molecule; and the directions most naturally suggested for this purpose, are the three axes respectively parallel to the co-ordinates. When the direct attraction is thus decomposed, the resulting forces, acting parallel to the axes, and directed to the planes from which the co-ordinates are reckoned, will be respectively,

$$\frac{dM(a-x)}{f^3}, \text{ parallel to the axis of } x,$$

$$\frac{dM(b-y)}{f^3}, \text{ parallel to the axis of } y,$$

$$\frac{dM(c-z)}{f^3}, \text{ parallel to the axis of } z.$$

Let A denote the accumulated amount of all the attractions, parallel to the axis of x; and, in like manner, let B and C denote the same things for the attractions parallel to the axes of y and z: then, by restoring the value of f, and writing $dx \cdot dy \, dz$ for its equivalent dM, there will be obtained,

$$A = \iiint \frac{dx \cdot dy \cdot dz \cdot (a-x)}{\left\{ (a-x)^2 + (b-y)^2 + (c-z)^2 \right\}^{\frac{1}{2}}}$$

$$B = \iiint \frac{dx \cdot dy \cdot dz \cdot (b-y)}{\left\{ (a-x)^2 + (b-y)^2 + (c-z)^2 \right\}^{\frac{1}{2}}} \quad (1)$$

$$C = \iiint \frac{dx \cdot dy \cdot dz \cdot (c-z)}{\left\{ (a-x)^2 + (b-y)^2 + (c-z)^2 \right\}^{\frac{1}{2}}}$$

where the several triple fluents must be extended to all the molecules that compose the mass of the solid.*

The expressions of A, B, and C, just found, are all integrable with respect to one of the variable quantities they contain. Thus A is integrable with respect to x: Let x' be the greatest value of x (y and z remaining constant) on the positive side of the plane of y and z, and x'' the greatest value, on the negative side of the same plane; then, the integration being performed, we shall get

$$A = \iint dy \cdot dz \cdot$$

$$\left\{ \frac{1}{\left\{ (a-x')^2 + (b-y)^2 + (c-z)^2 \right\}^{\frac{1}{2}}} - \frac{1}{\left\{ (a+x'')^2 + (b-y)^2 + (c-z)^2 \right\}^{\frac{1}{2}}} \right\}.$$

In this expression of A, the fluxion under the sign of double integration denotes the attraction which a prism of the matter of the solid, whose length is $x' + x''$ and its base $dy \cdot dz$, exerts on the attracted point, in the direction of the length of the prism.

If the plane, to which x is perpendicular, bisect the solid, as is the case of the principal sections of solids bounded by finite surfaces of the second order, then $x' = x''$: and as x' is nothing more than what x becomes at the surface of the solid, if we now suppose x, y, z to be three co-ordinates of a point in the surface, and, for the sake of brevity, put

* *Mécan. Céleste,* Tom. I. p. 3.

$$\Delta = \left\{ (a-x)^2 + (b-y)^2 + (c-z)^2 \right\}^{\frac{1}{2}}$$

$$\Delta' = \left\{ (a+x)^2 + (b-y)^2 + (c-z)^2 \right\}^{\frac{1}{2}}$$

then,

$$A = \iint dy \cdot dz \cdot \left\{ \frac{1}{\Delta} - \frac{1}{\Delta'} \right\}: \quad (2)$$

this double fluent is to be extended to all the points, or indefinitely small spaces $dy \cdot dz$, that compose the principal section of the solid made by the plane of y and z.

In like manner, if B and C be integrated; the first with respect to the variable y, and the second with respect to the variable z; two new expressions of these attractions will be obtained, exactly similar to the expression for A, that has just been investigated.

3. The general equation of a surface of the second order bounding a finite solid, is[*]

$$\frac{x^2}{k^2} + \frac{y^2}{k'^2} + \frac{z^2}{k''^2} = 1:$$

if the three quantities k, k', k'' be supposed to be all equal, then the solid will be a sphere; if two of them, as k' and k'' be equal, it will be a solid of revolution; and if all the three be unequal, it will be an ellipsoid, or a spheroid, having all its three principal sections ellipses. In what follows, we shall always suppose that k is the least of the three quantities k, k', k'', or the least of the semi-axes of the solid.

The general equation of the ellipsoid, will be satisfied by putting $x = k \cos. \varphi$, $y = k' \sin. \varphi \cos. \psi$, and $z = k'' \sin. \varphi \sin. \psi$; where φ and ψ denote two indeterminate angles. In order to substitute these values of x, y, and z in the formula (2), we must begin with taking the fluxion of y, on the sup-

[*] *Mecan. Celeste,* Tom. II. p. 7.

position that one of the indeterminate angles is constant; thus, if ψ be constant, then $dy = k \cos. \varphi \cos. \psi . d\varphi$: and, because y must be constant when z varies, we must make

$$dz = k'' \cos. \varphi \sin. \psi . d\varphi + k'' \sin. \varphi \cos. \psi . d\psi$$

$$0 = k' \cos. \varphi \cos. \psi . d\varphi - k' \sin. \varphi \sin. \psi . d\psi,$$

and, by exterminating $d\varphi$, we get $dz = \frac{k' \sin. \varphi}{\cos. \psi} . d\psi$. Thus, by substitution, the formula (2) will become

$$A = k' k'' \iint \sin. \varphi \cos. \psi . d\varphi . d\psi . \left\{ \frac{1}{\Delta} - \frac{1}{\Delta'} \right\}; \quad (3)$$

and,

$$\Delta = \left\{ (a - k \cos. \varphi)^2 + (b - k' \sin. \varphi \cos. \psi)^2 + (c - k'' \sin. \varphi \sin. \psi)^2 \right\}^{\frac{1}{2}}$$

$$\Delta' = \left\{ (a + k \cos. \varphi)^2 + (b - k' \sin. \varphi \cos. \psi)^2 + (c - k'' \sin. \varphi \sin. \psi)^2 \right\}^{\frac{1}{2}}:$$

the double fluent must be taken from $\varphi = 0$, to $\varphi = \frac{\varpi}{2}$ (ϖ denoting half the pheriphery of the circle, whose radius is 1), and from $\psi = 0$, to $\psi = 2\varpi$.

To obtain a further transformation of the last expression of A, we are now to determine the semi-axes of an ellipsoid, whose surface shall pass through the attracted point, and which shall have the same excentricities, and its principal sections in the same planes, as the given ellipsoid. Let h, h', h'' be the semi-axes required: then, because the attracted point is to be in the surface of the solid,

$$\frac{a^2}{b^2} + \frac{b^2}{b'^2} + \frac{c^2}{b''^2} = 1:$$

and, because the excentricities must be equal to those of the given ellipsoid, therefore $h'^2 - h^2 = k'^2 - k^2 = e^2$, and $h''^2 - h^2 = k''^2 - k^2 = e'^2$: hence

$$\frac{a^2}{b^2} + \frac{b^2}{b^2 + e^2} + \frac{c^2}{b^2 + e'^2} = 1;$$

an equation which now contains only one unknown quantity,

namely, h. It is plain that one value of h, and only one, may, in all cases, be determined from this equation. For, by taking h small enough, the function on the left hand side will become greater than any positive quantity how great soever; and by taking h great enough, the same function will become less than any positive quantity how small soever: and while h increases from 0, *ad infinitum*, the function continually decreases from being infinitely great to be infinitely little. Therefore there is only one ellipsoid, having the required conditions, whose surface will pass through the attracted point.* When h is determined, then $h' = \sqrt{h^2 + e^2}$, $h'' = \sqrt{h^2 + e'^2}$: and in consequence of the equation,

$$\frac{a^2}{b^2} + \frac{b^2}{b'^2} + \frac{c^2}{b''^2} = 1,$$

we may suppose, $a = h \cos. m$, $b = h' \sin. m \cos. n$, $c = h''$ sin. m sin. n.

Let these values of a, b, c be substituted in the last expressions for Δ and Δ': then

$$\Delta = \left\{ (h \cos. m - k \cos. \varphi)^2 + (h' \sin. m \cos. n - k' \sin. \varphi \cos. \psi)^2 \right.$$
$$\left. + (h'' \sin. m \sin. n - k'' \sin. \varphi \sin. \psi)^2 \right\}^{\frac{1}{2}}$$

$$\Delta' = \left\{ (h \cos. m + k \cos. \varphi)^2 + (h' \sin. m \cos. n - k' \sin. \varphi \cos. \psi)^2 \right.$$
$$\left. + (h'' \sin. m \sin. n - k'' \sin. \varphi \sin. \psi)^2 \right\}^{\frac{1}{2}} :$$

and because $h'^2 = h^2 + e^2$, $h''^2 = h^2 + e'^2$, $k'^2 = k^2 + e^2$, $k''^2 = k^2 + e'^2$, we shall readily obtain

$$\Delta = \left\{ h^2 - 2hk \cos. m \cos. \varphi - 2h' k' \sin. m \cos. n \sin. \varphi \cos. \psi \right.$$
$$+ 2h'' k'' \sin. m \sin. n \sin. \varphi \sin. \psi + k^2 + e^2 \sin.^2 m \cos.^2 n$$
$$\left. + e'^2 \sin.^2 m \sin.^2 n + e^2 \sin.^2 \varphi \cos.^2 \psi + e'^2 \sin.^2 \varphi \sin.^2 \psi \right\}^{\frac{1}{2}}$$

$$\Delta' = \left\{ k^2 + 2hk \cos. m \cos. \varphi - 2h'k' \sin. m \cos. n \sin. \varphi \cos. \psi \right.$$

$- 2h''k''$ sin. m sin. n sin. φ sin. $\psi + k^2 + e^2$ sin. ^2m cos. ^2n

$+ e'^2$ sin. ^2m sin. $^2n + e^2$ sin. $^2\varphi$ cos. $^2\psi + e'^2$ sin. $^2\varphi$ sin. $^2\psi \big\}^{\frac{1}{2}}$.

In these values of Δ and Δ', it is plain that the quantities h, h', h'' are alike concerned with the quantities k, k', and k'': and hence, by interchanging the semi-axes of the two ellipsoids, we may represent each of the expressions for Δ and Δ' in two forms, which, when expanded, are identical: thus

$\Delta = \big\{ (h \cos. m - k \cos. \varphi)^2 + (h' \sin. m \cos. n - k' \sin. \varphi \cos. \psi)^2$

$+ (h'' \sin. m \sin. n - k'' \sin. \varphi \sin. \psi)^2 \big\}^{\frac{1}{2}} = \big\{ (k \cos. m - h \cos.$

$\varphi)^2 + (k' \sin. m \cos. n - h' \sin. \varphi \cos. \psi)^2 + (k'' \sin. m \sin. n - h'$

sin. φ sin. $\psi)^2 \big\}^{\frac{1}{2}}$,

$\Delta' = \big\{ (h \cos. m + k \cos. \varphi)^2 + (h' \sin. m \cos. n - k' \sin. \varphi \cos. \psi)^2$

$+ (k' \sin. m \sin. n - k'' \sin. \varphi \sin. \psi)^2 \big\}^{\frac{1}{2}} = \big\{ (k \cos. m + h \cos.$

$\varphi)^2 + (k' \sin. m \cos. n - h' \sin. \varphi \cos. \psi)^2 + (k'' \sin. m \sin. n - h''$

sin. φ sin. $\psi)^2 \big\}^{\frac{1}{2}}$.

In the formula (3)

$$A = k'k'' \iint \sin. \varphi \cos. \varphi \,.\, d\varphi \,.\, d\psi \big\{ \tfrac{1}{\Delta} - \tfrac{1}{\Delta'} \big\},$$

the symbols Δ and Δ' express the distances of the attracted point, situated in the surface of the ellipsoid whose semi-axes are h, h', h'', and determined by the co-ordinates a, b, c, or h cos. m, h' sin. m cos. n, h'' sin. m sin. n, from the extremities of a prism of the matter of the ellipsoid first considered, parallel to the axis k, and having $k' k''$ sin. φ cos. $\varphi . d\varphi . d\psi$ for its base, and its length equal to $2k$ cos. φ: and, if we take a point in the surface of the last mentioned ellipsoid, that shall have k cos. m, k' sin. m cos. n, k'' sin. m sin. n (which we may denote by a', b', c') for its co-ordinates; and conceive a prism of the matter of the other ellipsoid, parallel to k and h, that

shall have $h' h'' \sin. \varphi \cos. \varphi . d\varphi . d\psi$ for its base, and its length equal to $2h \cos. \varphi$; then, it is a consequence of what has been shown above, that Δ and Δ' will likewise express the distances of the point, having a', b', c' for its co-ordinates from the extremities of this last prism. Therefore, if we put

$$A' = h'h'' \iint \sin. \varphi \cos. \varphi . d\varphi . d\psi \left\{ \frac{1}{\Delta} - \frac{1}{\Delta'} \right\} :$$

then will A' (when the double fluent is taken between the same limits as in the case of A) be equal to the attractive force which the ellipsoid of homogeneous matter, whose semi-axes are h, h', h'', exerts on the point, whose co-ordinates are $k \cos. m$, $k' \sin. m \cos. n$, $k'' \sin. m \sin. n$, or a', b', c', in the direction parallel to the axis h. For, in the formula for A, as the fluxion under the sign of double integration, denotes the attractive force of an indefinitely small prism of the matter of the ellipsoid, whose semi-axes are k, k', k'' upon the point whose co-ordinates are a, b, c, in the direction parallel to k and h; so, for the like reasons, in the formula for A', the fluxion under the same sign, will denote the attractive force of an indefinitely small prism of the matter of the ellipsoid, whose semi-axes are h, h', h'', upon the point whose co-ordinates are a', b', c': and therefore the two fluents, when extended to all the prisms that compose the ellipsoids, will denote the attractions of the whole masses upon the respective points, in the direction mentioned. Thus the attractions A and A' depend upon the same fluent, and they are manifestly in the same proportion as $k' k''$ is to $h' h''$.

And if we denote by B' and C' the attractive forces which the ellipsoid of homogeneous matter, whose semi-axes are h, h', h'' exerts on the point whose co-ordinates are a', b', c', in

the directions parallel to k' and k''; it may, in like manner, be shewn, that the attractions B and B' have the same proportion as kk'' has to hh''; and the attractions C and C', the same proportion as kk' to hh'.

The points in the surfaces of the two ellipsoids, which are determined by the co-ordinates, h cos. m, h' sin. m cos. n, h'' sin. m sin. n, or a, b, c, and k cos. m, k' sin. m cos. n, k'' sin. m sin. n, or a', b', c', may not improperly be called corresponding points of the surfaces: they are such points as are situated on the same sides of the planes of the principal sections, and have their co-ordinates respectively proportional to the axes to which they are parallel. This being premised, the result of the foregoing investigation may be enunciated, as in the following theorem:

" If two ellipsoids of the same homogeneous matter have
" the same excentricities, and their principal sections in the
" same planes; the attractions which one of the ellipsoids ex-
" erts upon a point in the surface of the other, perpendicularly
" to the planes of the principal sections, will be to the attrac-
" tions which the second ellipsoid exerts upon the correspond-
" ing point in the surface of the first, perpendicularly to the
" same planes, in the direct proportion of the surfaces, or
" areas, of the principal sections to which the attractions are
" perpendicular."

For the principal sections, being ellipses, their areas are proportional to the products of the semi-axes.

When the attracted point, of which the co-ordinates are a, b, c, is placed without the ellipsoid having k, k', k'' for its semi-axes; then the point, of which a', b', c' are the co-ordinates, is necessarily within the other ellipsoid: and, on account of the

relation which has been shewn to take place between the at-tractions of the two solids upon corresponding points in one another's surfaces, the case, when the attracted point is placed without an ellipsoid, is made to depend upon the case, when the attracted point is within the surface.

4. Let us now consider the formula (2) for the attractive force parallel to the axis k,

$$A = \iint dy \cdot dz \left\{ \frac{1}{\Delta} - \frac{1}{\Delta'} \right\}$$

on the supposition that the attracted point is within the ellip-soid. If $a = 0$ (that is, if the attracted point be in the plane of y and z) then $\frac{1}{\Delta} - \frac{1}{\Delta'} = 0$, for all values of x, y, and z: and, in this case, the whole attractive force A is evanescent, as it ought to be. For all other values of a, the expression $\frac{1}{\Delta} - \frac{1}{\Delta'}$, in the circumstances supposed, is plainly a finite po-sitive quantity: and, therefore, supposing b and c to be con-stant, and a to increase, we must infer that the attractive force A will receive finite increments, so long as the point deter-mined by the co-ordinates a, b, c, is within the ellipsoid. If this point be in the surface, then the variable ordinates x, y, z, when they belong to points indefinitely near to the attracted point, will approach indefinitely to an equality with a, b, c; and the corresponding values of $\frac{1}{\Delta} - \frac{1}{\Delta'}$, and, consequently, the fluxions of the force A, will become infinitely great; on which account the continuity of the function A is broken off. From what has now been observed, it follows, that we may substitute for the force A, its expansion in a series of the powers of a, provided we are careful not to extend the con-clusions obtained by reasoning from the nature of such series.

to the case when the attracted point is without the surface of the ellipsoid.

Let $R^2 = x^2 + (b-y)^2 + (c-z)^2$, then

$$\Delta = \left\{ R^2 + a(a-2x) \right\}^{\frac{1}{2}}$$

$$\Delta' = \left\{ R^2 + a(a+2x) \right\}^{\frac{1}{2}} :$$

and, if the function $\dfrac{1}{\Delta} - \dfrac{1}{\Delta'}$ be expanded into a series, the terminus generalis of that series will be

$$\pm \frac{1 \cdot 3 \cdot 5 \cdot 7 \ldots 2n-1}{2 \cdot 4 \cdot 6 \cdot 8 \ldots 2n} \cdot \frac{a^n (a+2x)^n - a^n (a-2x)^n}{R^{2n+1}} :$$

and, hence it is plain, that all the even powers of a will disappapear, and only the odd powers will remain. Now, the expansion of the force A cannot contain any of the powers of a, excepting those which enter into the series for $\dfrac{1}{\Delta} - \dfrac{1}{\Delta'}$: therefore, supposing the expansion of A to be arranged according to the powers of a, it will necessarily be of this form, *viz.*

$$A = A^{(1)} a + A^{(3)} a^3 + A^{(5)} a^5 + A^{(7)} a^7 +, \&c.:$$

where $A^{(1)}, A^{(3)}, A^{(5)}$, &c. are functions independent of a. The first of these coefficients, it is easy to prove, will be determined by this formula,

$$A^{(1)} = \iint \frac{2x \cdot dy \cdot dz}{\left\{ x^2 + (b-y)^2 + (c-z)^2 \right\}^{\frac{1}{2}}} : \quad (4)$$

and, with regard to the rest, they may be all shewn to depend on $A^{(1)}$, in consequence of an equation in partial fluxions, first noticed by LA PLACE, and derived from the nature of the functions under consideration. In effect, the truth of the following formulas will be established by merely performing the operations indicated, *viz.*

$$\left(\frac{dd \cdot \frac{1}{\Delta}}{da^2} \right) + \left(\frac{dd \cdot \frac{1}{\Delta}}{db^2} \right) + \left(\frac{dd \cdot \frac{1}{\Delta}}{dc^2} \right) = 0$$

$$\left(\frac{dd \cdot \frac{1}{\Delta'}}{da^2}\right) + \left(\frac{dd \cdot \frac{1}{\Delta'}}{db^2}\right) + \left(\frac{dd \cdot \frac{1}{\Delta'}}{dc^2}\right) = 0:$$

and hence it is easy to infer, that

$$\left(\frac{ddA}{da^2}\right) + \left(\frac{ddA}{db^2}\right) + \left(\frac{ddA}{dc^2}\right) = 0.$$

Substitute the series for A in this last equation, and let the coefficients of the several powers of a be equated to 0; and there will be obtained

$$A^{(3)} = -\frac{1}{2.3} \cdot \left\{ \left(\frac{ddA^{(1)}}{db^2}\right) + \left(\frac{ddA^{(1)}}{dc^2}\right) \right\}$$

$$A^{(5)} = -\frac{1}{4.5} \cdot \left\{ \left(\frac{ddA^{(3)}}{db^2}\right) + \left(\frac{ddA^{(3)}}{dc^2}\right) \right\}$$

$$A^{(7)} = -\frac{1}{6.7} \cdot \left\{ \left(\frac{ddA^{(5)}}{db^2}\right) + \left(\frac{ddA^{(5)}}{dc^2}\right) \right\},$$

&c.

Thus, all the other coefficients depend upon the coefficient of the first term, being derived from it by a repetition of the same operations: and when the general expression of $A^{(1)}$ shall be determined, the whole series will become known.

Resume the formula (4)

$$A^{(1)} = \int\!\!\int \frac{2x \cdot dy \cdot dz}{\left\{x^2 + (b-y)^2 + (c-z)^2\right\}^{\frac{3}{2}}};$$

and let

$$x = R \cos p$$
$$b - y = R \sin p \cos q$$
$$c - z = R \sin p \sin q,$$

then will $R = \left\{x^2 + (b-y)^2 + (c-z)^2\right\}^{\frac{1}{2}}$, express the line drawn from the foot of a to the point in the surface of the ellipsoid, of which x, y, z are the co-ordinates; p will be the angle which R makes with a; and q the angle which the plane drawn through R and a, makes with the plane of y and x. In

consequence of the equation of the solid, R is a function of the angles p and q: therefore, making p only variable, we shall have

$$- dy = \left\{ \left(\tfrac{dR}{dp} \right) \sin. p + R \cos. p \right\} \cos. q \cdot dp :$$

then, because y must be constant when z varies, we must make

$$- dz = \left\{ \left(\tfrac{dR}{dp} \right) \sin. p + R \cos. p \right\} \sin. q \cdot dp + \left\{ \left(\tfrac{dR}{dq} \right) \sin. q \right.$$
$$+ R \cos. q \left. \right\} \sin. p \cdot dq,$$

$$o = \left\{ \left(\tfrac{dR}{dp} \right) \sin. p + R \cos. p \right\} \cos. q \cdot dp + \left\{ \left(\tfrac{dR}{dq} \right) \cos. q - \right.$$
$$R \sin. q \left. \right\} \sin. p \cdot dq.$$

and by exterminating dp, we get

$$- dz = \frac{R \sin. p \cdot dq}{\cos. q} :$$

and hence, by substitution,

$$A^{(1)} = 2 \!\iint \left\{ \frac{\left(\tfrac{dR}{dp} \right)}{R} \cos. p \sin.{}^2 p + \cos.{}^2 p \sin. p \right\} dp \cdot dq ;$$

the fluent to be taken from $p = o$, to $p = \frac{\varpi}{2}$, and from $q = o$, $q = 2\varpi$.

The transformed formula for $A^{(1)}$ cannot be integrated, unless we substitute, in place of R, the function of the angles p and q, that is equal to it. Now, $x = R \cos. p$, $y = b - R \sin. p \cos. q$, $z = c - R \sin. p \sin. q$: let these values be substituted in the equation of the solid,

$$\frac{x^2}{k^2} + \frac{y^2}{k'^2} + \frac{z^2}{k''^2} = 1 ;$$

and, for the sake of simplicity, let

$$M = \frac{\cos.^2 p}{k^2} + \frac{\sin.^2 p \cos.^2 q}{k'^2} + \frac{\sin.^2 p \sin.^2 q}{k''^2},$$

$$N = \frac{b \sin. p \cos. q}{k'^2} + \frac{c \sin. p \sin. q}{k''^2},$$

$$D = 1 - \frac{b^2}{k'^2} - \frac{c^2}{k''^2}:$$

then

$$R^2 - 2 . \frac{N}{M} . R - \frac{D}{M} = 0.$$

This equation has two roots, *viz.*

$$R = \frac{\pm \sqrt{N^2 + MD} + N}{M};$$

and, because D is always positive when the attracted point is within the solid, as is here supposed, both these roots are real quantities, whatever be the angles p and q. Conceive the line R to be produced to meet the surface of the ellipsoid again below the plane of y and z, then, if the produced part be denoted by R', it is plain that R and R' will be the two roots of the above equation: and because R', although in an opposite direction, has the same angular position as R, we may substitute R' for R, in the expression for $A^{(1)}$: thus,

$$A^{(1)} = 2 \iint \left\{ \frac{\left(\frac{dR'}{dp}\right)}{R'} \cos. p \sin.^2 p + \cos.^2 p \sin. p \right\} dp . dq.$$

Therefore, by adding together the two values of $A^{(1)}$, and taking half the sum, we get

$$A^{(1)} = \iint \left\{ \left(\frac{\left(\frac{dR}{dp}\right)}{R} + \frac{\left(\frac{dR'}{dp}\right)}{R'} \right) \cos. p \sin.^2 p + 2 \cos.^2 p \sin. p \right\} dp.dq,$$

or

$$A^{(1)} = \iint \left\{ \frac{\left(\frac{d . RR'}{dp}\right)}{RR'} \cos. p \sin.^2 p + 2 \cos.^2 p \sin. p \right\} dp . dq:$$

the limits of this fluent being, as before, from $p = 0$ to $p = \frac{\varpi}{2}$, and from $q = 0$ to $q = 2\varpi$.

By the theory of equations $RR' = -\frac{D}{M}$: and, by substitution, the last expression of $A^{(1)}$ will become

$$A^{(1)} = \iint \left\{ - \frac{\left(\frac{dM}{dp}\right)}{M} \cos. p \sin.^2 p + 2 \cos.^2 p \sin. p \right\} dp . dq.$$

It is remarkable, that the last expression of $A^{(1)}$ does not contain either of the quantities b or c; for these do not enter into the function M: and hence we are to conclude that the value of $A^{(1)}$ is independent on these co-ordinates, and is the same for all points situated within the same principal section of the ellipsoid. Another inference is, that all the other co-efficients $A^{(3)}$, $A^{(5)}$, &c. of the expansion of the force A are severally equal to o, as is plain from the law which connects those quantities with one another, and with $A^{(1)}$: on this account the expansion alluded to will be reduced to its first term, and we shall have, simply,

$$A = A^{(1)} \times a.$$

The same considerations likewise suggest a new analytical expression of $A^{(1)}$; which, on account of its simplicity, and its immediate dependence on the figure and equation of the solid, seems to deserve the preference to every other: for, since it has been shewn that the value of $A^{(1)}$ is independent on the co-ordinates b and c, we may exterminate these quantities from the formula (4); and thus

$$A^{(1)} = \iint \frac{2x . dy . dz}{\left\{ x^2 + y^2 + z^2 \right\}^{\frac{3}{2}}}:$$

the fluent to be extended to the whole of the surface of the principal section made by the plane of y and z.

The same reasoning that has been applied to the determi-

nation of the attractive force A, it is evident, will apply equally to the attractions denoted by B and C: and, therefore, the attractions of an ellipsoid, acting perpendicularly to the planes of the principal sections, upon a point situated within the surface, are as follows, *viz.*

$$A = a \times \iint \frac{2x \cdot dy \ dx}{\left\{ x^2 + y^2 + z^2 \right\}^{\frac{3}{2}}}$$

$$B = b \times \iint \frac{2y \ dx \cdot dz}{\left\{ x^2 + y^2 + z^2 \right\}^{\frac{3}{2}}} \quad (5)$$

$$C = c \times \iint \frac{2z \ dx \cdot dy}{\left\{ x^2 + y^2 + z^2 \right\}^{\frac{3}{2}}} :$$

the several fluents to be extended to the whole of the surfaces of the principal sections, to which the attractions are perpendicular.

When the attracted point is without the ellipsoid, it becomes necessary, in the first place, to determine the semi-axes of another ellipsoid whose surface shall pass through the attracted point, and which shall have the same excentricities and its principal sections in the same planes, as the given ellipsoid: these semi-axes have been denoted by h, h', h'', and the formulas for computing them have already been given.* We must next determine the co-ordinates of the point in the surface of the given ellipsoid, that corresponds to the attracted point in the surface of the other ellipsoid: and, according to the definition that has been given of them, these co-ordinates, denoted by a', b', c' are thus found; $a' = a \times \frac{h}{b}$; $b' = b \times \frac{h'}{b'}$; $c' = c \times \frac{h''}{b''}$.† These things being determined, the attractions of the ellipsoid whose semi-axes are h, h', h'', upon the point whose

co-ordinates are a', b', c' (which is plainly within the solid) are as follows:

$$A' = a \times \frac{k}{b} \times \iint \frac{2x' \cdot dy' \cdot dz'}{\left\{ x'^2 + y'^2 + z'^2 \right\}^{\frac{3}{2}}}$$

$$B' = b \times \frac{k'}{b'} \times \iint \frac{2y' \cdot dx' \cdot dz'}{\left\{ x'^2 + y'^2 + z'^2 \right\}^{\frac{3}{2}}}$$

$$C' = c \times \frac{k''}{b''} \times \iint \frac{2z' \cdot dx' \cdot dy'}{\left\{ x'^2 + y'^2 + z'^2 \right\}^{\frac{3}{2}}} :$$

where x', y', z' are the three co-ordinates of a point in the surface of the ellipsoid, whose semi-axes are h, h', h''. To determine the attractions of the given ellipsoid upon the given point, we have now only to apply the theorem demonstrated in § 3; and so,

$$A = a \times \frac{kk'k''}{bb'b''} \cdot \iint \frac{2x' \cdot dy' \cdot dz'}{\left\{ x'^2 + y'^2 + z'^2 \right\}^{\frac{3}{2}}}$$

$$B = b \times \frac{kk'k''}{bb'b''} \cdot \iint \frac{2y' \cdot dx' \cdot dz'}{\left\{ x'^2 + y'^2 + z'^2 \right\}^{\frac{3}{2}}} \qquad (6)$$

$$C = c \times \frac{kk'k''}{bb'b''} \cdot \iint \frac{2z' \cdot dy' \cdot dx'}{\left\{ x^2 + y'^2 + z'^2 \right\}^{\frac{3}{2}}}.$$

5. If we examine the expressions (5) for the attractions of an ellipsoid upon a point placed within the surface, it will readily appear that the coefficients, into which the co-ordinates of the attracted point are multiplied, are homogeneous functions of o dimensions of the semi-axes of the solid, these quantities rising to the same dimensions in the numerators of the functions, as in the denominators: and hence it is easy to infer, that the values of these coefficients depend only on the proportions of the semi-axes to one another, and not at all upon their absolute magnitudes. Therefore, if we conceive two ellipsoids of the same homogeneous matter, similar to

one another and similarly placed, whose surfaces envelop the
same attracted point; it is plain, from what has just been re-
marked, that the attractions of these ellipsoids upon the point
will be precisely equal. Thus it appears, that the matter in-
closed between the surfaces of the two solids, does not alter
the attractive force of the inner ellipsoid; which could not be
the case, unless the attraction of the superadded matter in any
one direction were precisely equal to the attraction of the
same matter in the contrary direction, so as to produce an
equilibrium of opposing forces. Hence we may extend to a
shell of homogeneous matter, bounded by any finite surfaces
of the second order, which are similar to one another and
similarly placed, what Sir Isaac Newton has demonstrated in
the like hypothesis for surfaces of revolution;* as in the fol-
lowing theorem:

" If a point be situated within a shell of homogeneous mat-
" ter, bounded by two finite surfaces of the second order,
" which are similar and similarly placed; then the attraction
" of the matter of the shell upon the point, in any one direc-
" tion, will be equal to, and destroy, the attraction of the same
" matter, in the opposite direction."

6. Nothing more is wanting to complete a theory of the
attractions of homogeneous ellipsoids, than to integrate the
fluxional expressions (5) already obtained. In the case of a
sphere, we have $k = k' = k''$, and $x^2 + y^2 + z^2 = k^2$: there-
fore

$$A = a \times \iint \frac{2x \cdot dy \cdot dz}{k^3} :$$

now $2x \cdot dy \cdot dz$ is equal to a prism of the matter of the solid,

whose length is $2x$ and its base $dy \cdot dz$; and hence $\iint 2x \cdot dy \cdot dz$, taken within the limits prescribed, is no other than the mass of the sphere $= \frac{4\varpi}{3} \cdot k^3$. Therefore

$$A = \frac{4\varpi}{3} \times a.$$

The same reasoning, it is evident, will apply to the remaining attractions B and C: and hence the attractions of a sphere upon a point within the surface, acting perpendicularly to the planes of any three great circles that intersect at right angles, are thus expressed,

$$A = a \times \frac{4\varpi}{3}$$
$$B = b \times \frac{4\varpi}{3}$$
$$C = c \times \frac{4\varpi}{3}.$$

These three forces compose a force, directed to the centre of the sphere, and equal to $\frac{4\varpi}{3} \times \sqrt{a^2 + b^2 + c^2}$: it is therefore directly proportional to the distance from the center.

For a point without the surface of a sphere, we have $h = h' = h'' = \sqrt{a^2 + b^2 + c^2}$: hence it is easy to infer, that the formulas (6) will become,

$$A = a \times \frac{\frac{4\varpi}{3} \cdot k^3}{(a^2 + b^2 + c^2)^{\frac{3}{2}}} = \frac{a \times M}{(a^2 + b^2 + c^2)^{\frac{3}{2}}}$$

$$B = b \times \frac{\frac{4\varpi}{3} \cdot k^3}{(a^2 + b^2 + c^2)^{\frac{3}{2}}} = \frac{b \times M}{(a^2 + b^2 + c^2)^{\frac{3}{2}}}$$

$$C = c \times \frac{\frac{4\varpi}{3} \cdot k^3}{(a^2 + b^2 + c^2)^{\frac{3}{2}}} = \frac{c \times M}{(a^2 + b^2 + c^2)^{\frac{3}{2}}}.$$

where $M = \frac{4\varpi}{3} \cdot k^3 = $ the mass of the sphere. These three forces compose a force $= \frac{M}{a^2 + b^2 + c^2}$, directed to the center:

this force is, therefore, directly as the mass, and inversely as the square of the distance from the center of the sphere.

For an ellipsoid in general, we have $x = k$ cos. φ, $y = k'$ sin. φ cos. ψ, $z = k''$ sin. φ sin. ψ: in order to transform the formulas (5), we must first compute the values of $dy . dz$, $dx . dz$, $dx . dy$. For this purpose, let the fluxion of y be taken, making φ the only variable, so that $dy = k'$ cos. φ cos. $\psi \times d\varphi$: then, because y must be constant in the expression of the force A, when z varies, we must make

$$dz = k'' \text{ cos. } \varphi \text{ sin. } \psi . d\varphi + k'' \text{ sin. } \varphi \text{ cos. } \psi . d\psi$$
$$o = k' \text{ cos. } \varphi \text{ cos. } \psi . d\varphi - k' \text{ sin. } \varphi \text{ sin. } \psi . d\psi,$$

and, by exterminating $d\varphi$, we get $dz = k'' \frac{\text{sin. } \varphi}{\text{cos. } \psi} \times d\psi$: therefore

$$dy . dz = k'k'' \text{ cos. } \varphi \text{ sin. } \varphi . d\varphi . d\psi.$$

Again, because the value of x depends only on the angle φ, we have $dx = -k$ sin. $\varphi . d\varphi$: and, by taking the fluxions of y and z relatively to the variable ψ, we have $dy = -k'$ sin. φ sin. $\psi . d\psi$, $dz = k''$ sin. φ cos. $\psi . d\psi$: therefore,

$$dx . dy = kk' \text{ sin. }^2\varphi \text{ sin. } \psi . d\varphi . d\psi$$
$$dx . dz = kk'' \text{ sin. }^2\varphi \text{ cos. } \psi . d\varphi . d\psi;$$

in these expressions the sign —, which stands before the values of dx and dy, has been neglected: for that sign marks only that x and y decrease when the angles φ and ψ increase, and does not affect the absolute magnitudes of the fluents, which are alone the subjects of our research. Observing that $k'^2 = k^2 + e^2$, and $k''^2 = k^2 + e''^2$, the formulas (5) will now become by substitution.

$$A = a \times 2kk'k'' \times \iint \frac{\cos.^2\varphi \cdot \sin.\varphi \cdot d\varphi \cdot d\psi}{\left\{k^2 + e^2 \sin.^2\varphi \cos.^2\psi + e'^2 \sin.^2\varphi \sin.^2\psi\right\}^{\frac{3}{2}}}$$

$$B = b \times 2kk'k'' \times \iint \frac{\sin.^3\varphi \cos.^2\psi \cdot d\varphi \cdot d\psi}{\left\{k^2 + e^2 \sin.^2\varphi \cos.^2\psi + e'^2 \sin.^2\varphi \sin.^2\psi\right\}^{\frac{3}{2}}}$$

$$C = c \times 2kk'k'' \times \iint \frac{\sin.^3\varphi \sin.^2\psi \cdot d\varphi \cdot d\psi}{\left\{k^2 + e^2 \sin.^2\varphi \cos.^2\psi + e'^2 \sin.^2\varphi \sin.^2\psi\right\}^{\frac{3}{2}}} :$$

the several fluents to be taken from $\varphi = 0$ to $\varphi = \frac{\varpi}{2}$, and from $\psi = 0$ to $\psi = 2\varpi$.

Let

$$Q = \iint \frac{\sin.\varphi \cdot d\varphi \cdot d\psi}{\left\{k^2 + e^2 \sin.^2\varphi \cos.^2\psi + e'^2 \sin.^2\varphi \sin.^2\psi\right\}^{\frac{1}{2}}} :$$

then the last values of A, B, and C will be expressed by the partial fluxions of Q, as follows:

$$A = a \times 2kk'k'' \times \left\{ -\frac{1}{k}\left(\frac{dQ}{dk}\right) + \frac{1}{e}\left(\frac{dQ}{de}\right) + \frac{1}{e'}\left(\frac{dQ}{de'}\right) \right\}$$

$$B = b \times 2kk'k'' \times -\frac{1}{e}\left(\frac{dQ}{de}\right)$$

$$C = c \times 2kk'k'' \times -\frac{1}{e'}\left(\frac{dQ}{de'}\right).$$

For the sake of brevity, let $\rho^2 = e^2 \cos.^2\psi + e'^2 \sin.^2\psi$: then

$$-\left(\frac{dQ}{dk}\right) = \iint \frac{k \sin.\varphi \cdot d\varphi \cdot d\psi}{(k^2 + \rho^2 \sin.^2\varphi)^{\frac{3}{2}}} :$$

and, by integrating relatively to φ,

$$-\left(\frac{dQ}{dk}\right) = \int \frac{d\psi}{k^2 + \rho^2} \cdot \left\{ 1 - \frac{k \cos.\varphi}{(k^2 + \rho^2 \sin.^2\varphi)^{\frac{1}{2}}} \right\} :$$

and, by taking the whole fluent from $\varphi = 0$ to $\varphi = \frac{\varpi}{2}$, and restoring the value of ρ^2,

$$-\left(\frac{dQ}{dk}\right) = \int \frac{d\psi}{k^2 + e^2 \cos.^2\psi + e'^2 \sin.^2\psi} :$$

Let $\tau = \left(\frac{k^2 + e'^2}{k^2 + e^2}\right)^{\frac{1}{2}} \times \frac{\sin.\psi}{\cos.\psi}$; then, by substitution,

$$-\left(\frac{dQ}{dk}\right) = \frac{1}{(k^2 + e^2)^{\frac{1}{2}}(k^2 + e'^2)^{\frac{1}{2}}} \cdot \int \frac{d\tau}{1 + \tau^2},$$

and, by integrating from $\psi = 0$ to $\psi = 2\varpi$,

$$-\left(\frac{dQ}{dk}\right) = \frac{2\varpi}{(k^2 + e^2)^{\frac{1}{2}}(k^2 + e'^2)^{\frac{1}{2}}}:$$

hence

$$Q = 2\varpi \times \int \frac{-dk}{(k^2 + e^2)^{\frac{1}{2}}(k^2 + e'^2)^{\frac{1}{2}}}:$$

the fluent to be taken so as to vanish when k is infinitely great; because Q decreases, when k increases, and the former quantity is infinitely small, when the latter is infinitely great. From this value of Q, we get

$$-\frac{1}{e}\left(\frac{dQ}{de}\right) = 2\varpi \times \int \frac{-dk}{(k^2 + e^2)^{\frac{3}{2}}(k^2 + e'^2)^{\frac{1}{2}}}$$

$$-\frac{1}{e'}\left(\frac{dQ}{de'}\right) = 2\varpi \times \int \frac{-dk}{(k^2 + e^2)^{\frac{1}{2}}(k^2 + e'^2)^{\frac{3}{2}}}$$

$$-\frac{1}{k}\left(\frac{dQ}{dk}\right) = \frac{1}{k} \cdot \frac{1}{(k^2 + e^2)^{\frac{1}{2}}(k^2 + e'^2)^{\frac{1}{2}}}.$$

and from these it is easy to infer that

$$-\frac{1}{k}\left(\frac{dQ}{dk}\right) + \frac{1}{e}\left(\frac{dQ}{de}\right) + \frac{1}{e}\left(\frac{dQ}{de'}\right) = 2\varpi \times \int \frac{-dk}{k^2(k^2 + e^2)^{\frac{1}{2}}(k^2 + e'^2)^{\frac{1}{2}}}:$$

therefore, if $M = \frac{4\varpi}{3} \cdot kk'k'' =$ the mass of the ellipsoid, the last formulas for A, B, C will become, by substitution,

$$A = 3aM \times \int \frac{-dk}{k^2(k^2 + e^2)^{\frac{1}{2}}(k^2 + e'^2)^{\frac{1}{2}}}$$

$$B = 3bM \times \int \frac{-dk}{(k^2 + e^2)^{\frac{1}{2}}(k^2 + e'^2)^{\frac{1}{2}}} \quad (7)$$

$$C = 3cM \times \int \frac{-dk}{(k^2 + e^2)^{\frac{1}{2}}(k^2 + e'^2)^{\frac{1}{2}}}.$$

all these different fluents are to be conceived, as beginning to increase when k is infinitely great, and are to be extended till k has decreased, so as to be equal to the least of the semi-axes of the ellipsoid. In the general case of the problem, the expressions that have been obtained transcend the limits of the ordinary analysis; and their integration requires the introduction of other quantities besides algebraic expressions and circular arcs and logarithms. They belong to the class of elliptical transcendants; a branch of the mathematics which has been

very successfully cultivated, and is fertile in resources and methods that are applicable to every particular instance.

The fluents in the formulas (6) for a point without the surface, are derived from the ellipsoid whose semi-axes are h, h', h'', in the same manner as the fluents already considered are derived from the given ellipsoid: and, because $\frac{hh'h''}{bb'b''}$ is equal to the mass of the latter solid, divided by the mass of the former one, it is easy to infer that we have only to substitute h for k in the fluents of the formulas (7), to obtain the expressions of the attractions of the given ellipsoid upon a point without the surface. Thus the two cases, when the attracted point is within the solid or in the surface, and when it is without the solid, differ only in the limits of the fluents: in the former case, the fluents, beginning when the variable quantity is infinitely great, are to be extended till it has decreased, so as to be equal to the least of the semi-axes of the given ellipsoid; and, in the latter case, the fluents are to be extended only till the variable quantity has decreased, so as to be equal to h, the least of the semi-axes of the ellipsoid, whose surface passes through the attracted point. In the former case, the values of the fluents are the same for all points within the ellipsoid, and in its surface; in the latter case, these values depend upon the position of the attracted point.

The preceding formulas, being founded on the most general hypothesis, are applicable to all figures bounded by finite surfaces of the second order. The case of the sphere, which corresponds to the supposition that the excentricities e^2 and e'^2 are both evanescent, has already been considered, and, as it is attended with no difficulty, it needs not be again discussed;

but the two cases of solids of revolution, that of the oblate and oblong spheroids, are deserving of particular attention.

In the oblate spheroid, the two greater semi-axes k' and k'' are equal to one another; and, therefore, it corresponds to the supposition of $e^2 = e'^2$. In this case the formulas (7) will become

$$A = 3aM . \int \frac{- dk}{k^2 (k^2 + e^2)}$$

$$B = 3bM . \int \frac{- dk}{(k^2 + e^2)^2}$$

$$C = 3cM . \int \frac{- dk}{(k^2 + e^2)^2} :$$

these expressions may be all integrated by the ordinary methods, and thus we get

$$A = \frac{3aM}{e^3} . \left\{ \frac{e}{k} - \text{arc. tan.} \frac{e}{k} \right\}$$

$$B = \frac{3bM}{2e^3} . \left\{ \text{arc. tan.} \frac{e}{k} - \frac{\frac{e}{k}}{1 + \frac{e^2}{k^2}} \right\}$$

$$C = \frac{3cM}{2e^3} . \left\{ \text{arc. tan.} \frac{e}{k} - \frac{\frac{e}{k}}{1 + \frac{e^2}{k^2}} \right\}.$$

The formulas express the attractions of an oblate spheroid upon a point within the surface or in it, acting parallel to a, b, c, the co-ordinates of that point, of which a is parallel to the axis of revolution.

When the attracted point is without the surface, we have only to compute h, the semi-axis of revolution of the spheroid, whose surface passes through the attracted point, and to substitute for k in the last formulas, in order to have the expressions of the attractions sought: and it is to be remarked, that the same manner is used in the general case of the

ellipsoid rises to the third degree, is, in this case, depressed to a quadratic. In effect, the equation for h,[*] when $e^2 = e''^2$, becomes

$$\frac{a^2}{b^2} + \frac{b^2 + c^2}{b^2 + e^2} = 1,$$

whence

$$h'' - (a^2 + b^2 + c^2 - e^2)\, h^2 = a^2 e^2,$$

and so

$$2h^2 = a^2 + b^2 + c^2 - e^2 + \sqrt{(a^2 + b^2 + c^2 - e^2)^2 + 4a^2 e^2}.$$

In the oblong spheroid, one of the semi-axes k' and k'' must be made equal to the least semi-axis k, which corresponds to the suppposition of $e'^2 = 0$. In this case, the formulas (7) will become

$$A = 3a M \cdot \int \frac{-dk}{k^3 (k^2 + e^2)^{\frac{1}{2}}}$$

$$B = 3b M \cdot \int \frac{-dk}{k (k^2 + e^2)^{\frac{3}{2}}}$$

$$C = 3c M \cdot \int \frac{-dk}{k^3 (k^2 + e^2)^{\frac{1}{2}}}.$$

In these expressions k is the radius of the equatorial circle of the spheroid, and not the semi-axis of revolution, which is $= \sqrt{k^2 + e^2}$: and if we change k to denote the semi-axis of revolution, which requires that $\sqrt{k^2 - e^2}$ be substituted for k; and, for the sake of uniformity with the formulas for the oblate spheroid, likewise interchange a and b, and A and B, in order that a may denote the ordinate parallel to the axis of revolution, and that A may express the attractive force in the same direction; then, the last expressions will become

$$A = 3a M \cdot \int \frac{-dk}{k^2 (k^2 - e^2)}$$

$$B = 3b M \cdot \int \frac{-dk}{(k^2 - e^2)^{\frac{1}{2}}}$$

$$C = 3c M \cdot \int \frac{-dk}{(k^2 - e^2)^{2}}.$$

[*] Page 351.

which differ from the formulas for the oblate spheroid only in the sign of e^2, as, it is manifest, ought to be the case. By integrating, we get

$$A = \frac{3a\mathrm{M}}{e^3} \cdot \left\{ \tfrac{1}{2} \cdot \text{hyp. log.} \left(\frac{k+e}{k-e} \right) - \frac{e}{k} \right\}$$

$$B = \frac{3b\mathrm{M}}{2e^3} \cdot \left\{ \frac{\frac{e}{k}}{1 - \frac{e^2}{k^2}} - \tfrac{1}{2} \cdot \text{hyp. log.} \left(\frac{k+e}{k-e} \right) \right\}$$

$$C = \frac{3c\mathrm{M}}{2e^3} \cdot \left\{ \frac{\frac{e}{k}}{1 - \frac{e^2}{k^2}} - \tfrac{1}{2} \cdot \text{hyp. log.} \left(\frac{k+e}{k-e} \right) \right\}.$$

These formulas express the attractions of an oblong spheroid upon a point within the surface or in it; acting parallel to a, b, c, the co-ordinates of that point, of which a is parallel to the axis of revolution.

When the attracted point is without the spheroid, we must first compute h, the semi-axis of revolution of the spheroid, whose surface passes through the attracted point; and for this purpose we have the following expression, *viz.*

$$2h^2 = a^2 + b^2 + c^2 + e^2 + \sqrt{(a^2 + b^2 + c^2 + e^2)^2 - 4a^2 e^2};$$

observing that a is the ordinate parallel to h: then the attractions required will be found merely by substituting h for k in the formulas for the case when the attracted point is within the spheroid.

XXI. *Observations on Albumen, and some other Animal Fluids; with Remarks on their Analysis by electro-chemical Decomposition. By Mr.* William Brande, *F. R. S. Communicated by the Society for the Improvement of Animal Chemistry.*

Read June 15, 1809.

SECTION I.

Observations on Mucus and on the Composition of liquid Albumen.

THE results obtained from the chemical analysis of the intervertebral fluid of the squalus maximus, an account of which is annexed to Mr. HOME's paper " On the Nature of the in-" tervertebral Substance in Fish and Quadrupeds,"* led me to undertake a series of experiments on mucus, in order to examine the properties of that secretion in its pure state, and to ascertain how far it might be capable of conversion into modifications of gelatine and albumen.

1. Saliva was the first source of mucus to which I directed my attention.

In order to separate the albumen, which Dr. BOSTOCK's analysis has shewn it to contain,† it was agitated for a short time with an equal quantity of pure water; the solution was then boiled and filtered. I considered the clear fluid, which had

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* Philosophical Transactions, 1809.

† NICHOLSON's Journal, Vol. XIV. page 149.

passed the filter, as a solution of nearly pure mucus; but found, on applying to it the tests of nitrate of silver, and acetate of lead, that it still contained a very considerable proportion of saline matter. The precipitate consisted of muriate and phosphate of silver and lead, in combination with a little animal matter, the odour of which was perceptible on exposing it to heat after it had been washed and dried.

One thousand grains of saliva, afforded by careful evaporation in a water bath, a residuum weighing one hundred and eighty grains, from which twenty grains of saline matter, consisting of phosphate of lime and muriate of soda, were obtained by incineration.

2. The mucus from the trachea, and that of the oyster were next examined; but here the proportion of saline matter was greater than in the former case, although no traces of albumen could be detected by the usual tests of heat, alcohol, and acids.

Finding, therefore, that the re-agents employed to detect mucus,* act principally upon the salts which it contains, and not merely upon the secretion itself, it became an object of some importance to find out a method of depriving it of its saline ingredients, by such means as should not affect the mucus. Decomposition by electricity immediately occurred to me, as the most likely means of attaining the object I had in view.

For this purpose, I procured three glass cups, each capable of holding rather more than a measured half ounce of water; one of these was filled with a mixture of equal parts of saliva

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* Nitrate of silver and acetate of lead. Vide THOMSON's System of Chemistry, Vol. V. page 504, 3d edition; and NICHOLSON's Journal, XI—251.

and pure water; this was connected with the other two, containing pure water, by filaments of moistened cotton. The water in one of the cups was rendered positive, that in the other negative, by a VOLTAIC battery of one hundred and twenty four inch double plates, charged with a solution of nitro-muriatic acid, in the proportion of one part of the mixed acid to thirty parts of water.* By continuing this process, I hoped to decompose the saline ingredients of the saliva, to collect the acid matter in the positive, and the alkaline matter in the negative cup, and thus to leave the mucus and albumen in the centre vessel (free from the salts which they contain in their natural state), and to have separated them by boiling distilled water, which would then have afforded a solution of pure mucus.,

When the action of the battery had been continued for about ten minutes, a considerable quantity of a white substance, surrounded, and adhered to, the cotton on the negative side of the circuit, whereas on the positive side no such effect had taken place.

I could not at first account for this appearance, conceiving that if it depended on the coagulation of albumen held in solution in the saliva, it would have taken place at the positive pole, in consequence of the acid there separated.

To ascertain this point, an experiment was made on the albumen of an egg.

When the conductors from the same battery were brought within two inches of each other in this fluid, an immediate and rapid coagulation took place at the negative wire, while

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* It was conceived, that this electrical power, though sufficient for the decomposition of the salts, would not materially affect the animal matter.

only a thin film of albumen collected at the positive wire, where its appearance was readily accounted for, by the separation of a little acid, which re-acting on the albumen would render it solid; but the cause of the abundant coagulation at the negative pole was not so obvious.

This result I mentioned to Mr. DAVY, who immediately offered an explanation of it, by supposing the fluidity of albumen to depend upon the presence of alkaline matter, the separation of which, at the negative pole, would cause it to assume a solid form. I had only to follow up this idea, and shall proceed to state the principal experiments which were undertaken to establish so probable an opinion.*

1. When coagulated albumen, cut into small pieces, is boiled in distilled water, it imparts a viscidity to that fluid, shewing that something is retained in solution.

Two hundred grains of the coagulated albumen of an egg, were repeatedly washed and triturated, in four ounces of distilled water, which was afterwards separated by a filter, and evaporated to about one fourth of its original bulk. It was then examined by the usual tests, and was found evidently alkaline; it converted the yellow of turmeric to a pale brown, and restored the blue colour to litmus paper, reddened by vinegar; but it did not appear to effervesce on the addition of a dilute acid.

On evaporating this alkaline fluid to dryness, by a gentle heat, a viscid substance, soluble in water, was obtained. This solution was rendered slightly turbid by an acid; and by the

* On referring afterwards to Dr. THOMSON's System of Chemistry (Vol. V. page 491), I find that a very similar explanation of the coagulation of albumen has been offered by that author, which the following experiments will likewise confirm.

application of electricity, from sixty four inch double plates, a copious coagulation took place at the negative pole.

So that water, in which the coagulated white of egg has been boiled, is in fact an extremely dilute alkaline solution of albumen.

This enables us also to explain why albumen becomes co-agulated simply by heat.

When the coagulated white of egg is cut into pieces, a small quantity of a brown viscid fluid gradually separates from it, as has been observed by Dr. Bostock in his paper on the primary animal fluids.* This I find to consist principally of an alkaline solution of albumen. It reddens turmeric, and coagulates abundantly on the application of negative electricity.

It appears, therefore, that the white of egg, in its fluid state, is a compound of albumen, with alkali and water; that when heat is applied to it, the affinities existing between these bodies are modified; that the alkali, before in chemical combination with the albumen, is transferred to the water, and that this separation causes the coagulation of the albumen: the aqueous alkaline solution which is thus formed, re-acts upon the co-agulated albumen, of which it dissolves a small portion, and then appears in the form of the brown viscid fluid already noticed.

The coagulation of albumen by alcohol and by acids, may be explained by a reference to the principles already laid down.

1. Five hundred grains of the white of egg were agitated with two ounces of pure alcohol; an immediate coagulation resulted, which was rendered more perfect by the application of a very gentle heat. The liquid was separated from the

* Nicholson's Journal, Vol. XI—246.

coagulum by filtration, and evaporated to half its bulk; when the usual tests were now applied, alkaline matter was abundantly indicated.

In this instance then, the albumen in passing from the liquid to the solid state, gives its alkali to the alcohol.[*]

2. When acids are applied to albumen, these effect its coagulation from the same cause: they render it more rapidly and more perfectly solid, on account of their superior affinity for the alkali.

The following experiments were instituted with a view to ascertain the nature and quantity of the alkaline matter which exists in liquid albumen.

1. Five hundred grains of the liquid white of egg were mixed with two ounces of distilled water, and exposed for half an hour to a temperature of 212°. The fluid was then separated by a filter, and the coagulated albumen cut into small pieces, and repeatedly washed with boiling distilled water. The filtrated fluid was evaporated to half an ounce by measure; it had a saline taste, it was somewhat turbid, and slightly alkaline; on cooling, it gradually deposited a few flakes of albumen: it was electrified positively in a small glass cup, connected by washed cotton to another similar vessel containing a little distilled water, negatively electrified by one hundred four inch plates, charged with a solution of nitro-muriatic acid of the same strength as that employed in a former experiment, fresh portions of water being occasionally added in order to compensate for the loss by its decomposition.

[*] When albumen is coagulated by alcohol, it does not become so perfectly solid as in most other instances, because the separation effected by the relative affinities is not so complete.

When the electrization had been carried on in this way for one hour, the cups were removed, and their contents examined.

The fluid in the negatively electrified cup acted rapidly on turmeric, rendering it deep brown. On evaporation and subsequent exposure to a low red heat, it afforded a residuum weighing 5.5 grains, which had the properties of soda, in a state approaching to purity.

The positive cup contained a little coagulated albumen, and an acid which was principally, if not entirely the muriatic, was held in solution by the water: it gave a very copious precipitate with nitrate of silver, which became speedily black on exposure to light. When saturated with carbonate of soda, and evaporated, it afforded a salt in small cubic crystals, from which the fumes of muriatic acid were developed by the action of the sulphuric.

This experiment shews that, exclusive of soda in an uncombined state, fluid albumen contains some muriate of soda.* We learn, from the experiments of Mr. HATCHETT, that minute quantities of other saline bodies are likewise present.†

In the foregoing experiments, I had generally employed from sixty to three hundred four inch double plates of copper and zinc, but in subsequent researches, made with a view of

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* May not a submuriate of soda exist in fluid albumen?

† After the destructive distillation of coagulated, dry, semi-transparent albumen, there remained " a spongy coal of very difficult incineration; as towards the end of " the process, it appeared vitrified and glazed with a melted saline coat, which was, " however, easily dissolved by water. The residuum was again exposed to a long " continued red heat, and again treated with water, till, at length, a few scarcely " visible particles remained, which as far as such a small quantity would permit to " be ascertained, proved to be phosphate of lime. The portion dissolved by water.

ascertaining the action of lower powers, the effects of which I shall afterwards relate, I find that a battery of twenty-four three inch double plates is sufficient to effect a perfect coagulation at the negative pole, even where the albumen is diluted with so large a quantity of water, as not to be detected by the usual tests.

SECTION II.

Observations on the Composition of some animal Fluids containing Albumen.

Finding, from the experiments detailed in the preceding section, that albumen may exist in such states of combination, as not to be detected by the usual tests, but separable by electrical decomposition, I was induced to apply this mode of analysis to the examination of animal fluids in general.

1. *Saliva.*

When saliva is boiled in water, a few flakes of coagulated albumen are deposited; but this is by no means the whole quantity of albumen contained in the secretion, for on applying the test of negative electricity to the filtered fluid obtained after the separation of the albumen by heat, a copious coagulation and separation of alkali, is produced at the negative pole. A large portion of albumen may therefore exist in a fluid,

which was by much the most considerable), consisted principally of carbonate, with a small quantity of phosphate of soda.

grains of dry albumen afforded 74.50 grains of coal, of which

some Observations on the Com-

incapable of separation by heat, and in the present instance, not to be detected even by acids, these re-agents producing no effect on the filtered solution, just alluded to.

2. *Mucus of the Oyster.*

The solution of mucus obtained by agitating oysters in water, exhibits to the usual tests no traces of albumen; but when acted upon by electricity from the VOLTAIC battery, a considerable and rapid coagulation takes place at the negatively electrified wire.

3. *Mucus of the Trachea, &c.*

The other varieties of mucus, as from the trachea, the nose, &c. agree with the former, in affording abundance of albumen by electric decomposition, whereas scarcely any traces of that substance can be detected by the tests of acids, heat, or alcohol.

In these experiments, alkaline matter was always evolved at the negative, and acid at the positive wire. Minute researches, made with a view of ascertaining the nature of the alkaline and acid matter thus evolved, shewed the former to consist of soda, with traces of lime; the latter of muriatic acid, with traces of phosphoric acid, in the cases of saliva, and mucus of the trachea and nose: the mucus of the oyster afforded only soda and muriatic acid.

On examining the proportions of alkali and acid, the former seemed always to predominate, although in the original fluids, no traces of uncombined alkali (as in the white of egg) are to be detected.

These results lead to new ideas respecting the composition

of mucus: Is it a peculiar combination of muriate of soda and albumen? or may it not be a compound of soda and albumen, in which the alkali is not separable by the usual modes of analysis, but which yields to the superior decomposing energy of electricity?

4. *Bile.*

An immediate coagulation took place in this secretion, at the negative conductor, the albumen being tinged throughout of a green colour, arising from the colouring matter at the same time separated.

The relative proportion of albumen, separable by electricity from different specimens of ox-bile, was found to be liable to considerable variation, so that a detailed analysis of this fluid, cannot be generally depended upon. I have found the albumen in bile to vary in quantity from 0.5 to three per cent., and it is somewhat remarkable, that where there is a small quantity of albumen, there likewise the proportion of the resinous matter of bile is relatively small.

The electro-chemical decomposition of this fluid, affords, besides the results just mentioned, a considerable quantity of soda at the negative pole; and at the positive pole, a mixture of muriatic and phosphoric acids.

5. *Milk.*

In this fluid, the separation of albuminous matter at the negative pole, is equally evident, though not so rapid, as in most other cases. The conductors from sixty four inch double plates, highly charged, and immersed within four inches of each other in three ounces of cows milk, during one hour, produced the appearance of curds and whey, the principal part

of the curd being collected in the neighbourhood of the negative wire, and but little at the positive wire. When this experiment was so conducted, as to collect the products in separate vessels, the predominating ingredients in the contents of the negative cup, were soda, and traces of lime; and in the positively electrified vessel, a mixture of muriatic and phosphoric acids.

After such decomposition of milk, the serum still affords sugar of milk.

6. *The Liquor of the Amnios.*

An opportunity having offered of examining this secretion, from the human subject, in its pure and fresh state, I shall mention the general results of its analysis.

The liquor of the amnios is almost perfectly transparent, but on exposure to air becomes gradually turbid, and deposits a white flaky matter. It renders tincture of violets green, and while perfectly fresh does not affect litmus; but sulphuretted hydrogen is soon evolved from it, and then it slightly reddens litmus. When heated, it becomes turbid, and lets fall flakes of coagulated albumen. Acids render it slightly turbid from the same cause.

Alkalies produce no change, unless when added in considerable excess: the odour of ammonia is then perceptible.

Electrical analysis afforded albumen and soda at the negative pole, and muriatic acid at the positive pole. Hence we learn, that the liquor of the amnios has the properties of a dilute solution of liquid albumen.*

* The difference in the results of the analysis given in the text, and that of Vauquelin and Buniva, most probably arises from the liquor of the amnios examined by those chemists, not having been perfectly recent, and perhaps mixed with other secretions. Vide *Annales de Chimie*, XXXIII. p. 270.

7. *Pus.*

In the pus of a healthy sore, coagulation took place at both poles; most abundantly, however, at the negative pole. A slight degree of putrefaction having commenced in the pus which was examined, I did not pay particular attention to the other products of the experiment.

In concluding this section, it may be proper to remark, that the decomposition of liquid albumen by VOLTAIC electricity, takes place in different ways, according to the power employed. With a comparatively high electrical power, the coagulation goes on rapidly at the negative pole, and only very slowly at the positive pole; whereas, with an extremely low power, the coagulation is comparatively rapid at the positive surface, an alkaline solution of albumen surrounding the negative pole. Thus, when the conductors from twenty four four inch double plates, highly charged, were brought within half an inch of each other, in a dilute solution of albumen (consisting of one part of albumen to six of water), the coagulation was considerably more abundant at the negative than at the positive pole; but when the conductors were removed from each other to a distance of eight inches, or when they remained at half an inch, being connected with a battery of six four inch double plates only, the coagulation was only perceptible at the positive pole, in consequence of the acid there collected. Hence we may infer, that a rapid abstraction of alkali is necessary to the perfect coagulation of albumen, since, in the cases above alluded to, the albumen remains in solution.

XXII. *Hints on the Subject of animal Secretions.* *By* Everard Home, *Esq. F. R. S. Communicated by the Society for the Improvement of Animal Chemistry.*

Read June 22, 1809.

T HE brilliant discoveries of Mr. DAVY on the powers of electricity in producing chemical changes, suggested to me the

Dr. WOLLASTON's observations inserted in the Philosophical Magazine, were published after this paper had been laid before the Society.

I was led to the present investigation, while preparing my lectures on the Hunterian Museum, in which the secretions in different animals are to be considered. In September last, I engaged Mr. WILLIAM BRANDE to assist me in prosecuting the inquiry. In November, I communicated my opinions to Sir JOSEPH BANKS, and stated that I should bring them forward in my lectures; at that time Dr. YOUNG's Syllabus was not published, and Dr. WOLLASTON's opinions were unknown to me.

Dr. BERZELIUS, Professor of Chemistry at Stockholm, published a work on Animal Chemistry, in the year 1806, in the Swedish language, in which he states, in several places, that he believes the secretions in animals to depend upon the nerves, although he is unable to explain how the effect is produced. In proof of his opinion, the following experiment is adduced:

" Trace all the nerves leading to any secretory organ in a living animal, and divide " them, being careful to injure the blood-vessels and the structure of the organ " itself, as little as may be: notwithstanding the continued circulation of the blood, " the organ will as little secrete its usual fluid, as an eye deprived of its nerve can " see, or a muscle whose nerve has been divided can move. We may therefore easily " conceive, that any trifling alteration in the nerves of a gland, may materially affect " its secretion, the supply of blood being in every way perfect."

He says, the agency of the nerves in secretion has generally been disregarded, because our attention is only called to their secret mode of acting, when we discover the insufficiency of all other explanation. Dr. BERZELIUS's work was shown to me by Mr. DAVY while this paper was in the press.

idea that the animal secretions may be produced by the same means.

To prosecute this inquiry with every advantage, requires a knowledge of anatomy, physiology, and chemistry, rarely to be met with in the same person. I have therefore availed myself of the assistance of the different members of this Society, the object of which is the improvement of Animal Chemistry, their intimate acquaintance with these branches of science, renders them peculiarly fitted for such an undertaking.

It is one of the most important subjects to which Mr. DAVY's discoveries can be applied, and he has given it the consideration it deserves.

The VOLTAIC battery is met with in the torpedo and electrical eel, and although it is given only as a means of catching their prey, and defending themselves, and therefore not immediately applicable to the present inquiry, yet it furnishes two important facts, one, that a VOLTAIC battery can be formed in a living animal, the other, that nerves are essentially necessary for its management; for in these fish, the nerves connected with the electrical organs, exceed those that go to all the other parts of the fish, in the proportion of twenty to one. The nerves are made up of an infinite number of small fibres, a structure so different from that of the electric organ, that they are evidently not fitted to form a VOLTAIC battery of high power; but their structure appears to Mr. DAVY to adapt them to receive and preserve a small electrical

nerves arranged with muscles, so as to form a VOLTAIC battery, have a power of accumulating and commu-

nicating electricity, is proved by the well known experiment of taking the two hind legs of a vivaceous frog, immediately after they are cut off, laying bare the crural nerves, applying one of these to the exposed muscles of the other limb, and then when the circle is completed by raising the other crural nerve with a glass rod, and touching the muscle of the limb to which it does not belong, the muscles of both are excited to contractions.

There are several circumstances in the structure of the nerves, and their arrangements in animal bodies, which do not appear at all applicable to the purposes of common sensation, and whose uses have not even been devised. Among these are the plexuses in the branches of the par vagum which go to the lungs, and in the nerves which go to the limbs. The ganglions, which connect the nerves belonging to the viscera with those that supply the voluntary muscles, and the course of the nerves of the viscera which keep up a connexion among themselves in so many different ways.

The organs of secretion are principally made up of arteries and veins; but there is nothing in the different modes in which these vessels ramify, that can in any way account for the changes in the blood, out of which the secretions arise. These organs are also abundantly supplied with nerves.

With a view to determine how far any changes could be produced in the blood by electricity, at all similar to secretion, Mr. W. BRANDE, who has begun his career in animal chemistry with so much success, made the following experiments, in the suggestion of which Mr. DAVY afforded him every assistance.

Experiment 1. Middle of January, 1809.

The conductors from twenty four four inch double plates of copper and zinc, charged with a very weak solution of muriatic acid, were immersed in four ounces of blood, immediately on its having been withdrawn from a vein in the arm. The temperature of the blood was kept up at 100° during the experiment. The apparatus was so constructed, as to admit of the products at the negative and positive wires being separately collected and examined. When the electrization had been carried on for a quarter of an hour, all action seemed to have ceased. The blood which had surrounded the negative wire, was of a deep red colour and extremely alkaline; that surrounding the positive wire was slightly acid, and of a brighter hue.

In this experiment, the coagulation of the blood was not materially affected by the electrical power alluded to.

Experiment 2. 8th of February, 1809.

Finding it necessary to submit perfectly fluid blood to the action of electricity, the following experiment was undertaken with a view of keeping it the longest possible time in that state.

A deer having been pithed, the abdomen was immediately opened into, and a length of about four inches of a large vein in the meso-colon was detached from the neighbouring parts. Two small platina wires, connected in the usual way with three inch double plates, were inserted into this detached vein, and secured by ligatures, having their points about one inch from each other. The communication battery was kept up for one quarter of

an hour, a third ligature was then tied in the centre of the detached vein, in order to cut off the connection between the positive and negative ends. On removing the portion of the vein included by the ligatures, and containing the conductors, it was found that the gaseous products had forced out nearly the whole of the blood, at the part through which the wires were inserted; alkaline and acid matter were readily detected, but no new product could be discovered.

Finding the coagulation of the blood an insurmountable obstacle to the long continued electrical action, the serum only was employed in the following experiments.

Experiment 3. 10th of March, 1809.

The conductors from one hundred and twenty four inch double plates, highly charged, were brought within two inches of each other, in some recent serum of blood, obtained free from the colouring matter, by carefully pouring it off from the coagulum. Coagulated albumen was rapidly separated at the negative pole, and alkaline matter evolved: at the positive pole, a small quantity of albumen was gradually deposited, and litmus paper indicated the presence of acid. These are the effects produced by a high electrical power upon serum.

Experiment 4. 14th of April, 1809.

Was undertaken to ascertain the effect of a low power; a battery was employed, consisting of twelve four inch double plates of copper and iron. In this case, there was at first no appearance of coagulation at either pole; in five minutes, the positive wire became covered with a film of albumen, and in fifteen minutes a filament of about a quarter of an inch in

length was seen floating in the fluid, and adhering to the same
wir

Experiment 5. 6th of May, 1809.

Two small platina cups, connected by a large quantity of
cotton well washed, and each containing one ounce of serum,
were rendered positive and negative, by thirty double three
inch plates *very weakly* charged. The process was continued
during twenty-four hours. This power had not been sufficient
to produce coagulation at the negative pole. On examining
the fluid in the negative cup, it was found to consist principally
of an alkaline solution of albumen.

The fluid in the positive cup was rather turbid, it reddened
litmus, and was slightly acid to the taste. On standing, it
deposited a few flakes of albumen. When evaporated, it af-
forded saline matter, with excess of acid, (super salts.)

By these experiments it is ascertained, that a low negative
power of electricity separates from the serum of the blood an
alkaline solution of albumen; that a low positive power sepa-
rates albumen with acid, and the salts of the blood. That with
one degree of power, albumen is separated in a solid form,
with a less degree, it is separated in a fluid form.

From these facts, the following queries are proposed.

1st. That such decomposition of the blood by electricity,
may be as near an approach to secretion, as could be expected
to be produced by the artificial means at present in our

2d. That a weaker power of electricity, than any that can
be readily kept up by art, may be capable of separating from

the blood, the different parts of which it is composed, and forming new combinations of the parts so separated.

3d. That the structure of the nerves may fit them to have a low electrical power, which can be employed for that purpose, and as such low powers are not influenced by imperfect conductors, as animal fluids, the nerves will not be robbed of their electricity by the surrounding parts.

4th. That the discovery of an electrical power, which can separate albumen from the blood in a fluid state, and another that separates it in a solid state, may explain the mode in which different animal solids and fluids may be produced, since, according to Mr. HATCHETT's experiments, albumen is the principal material of which animal bodies are composed.

5. That the nerves of the torpedo may not only keep the electric organ under the command of the will, but charge the battery, by secreting the fluid between the plates, that is necessary for its activity.

6. As albumen becomes visibly coagulated, by the effect produced from twelve four inch double plates of copper and iron, a power much too low to affect even the most delicate electrometer, may not this be occasionally employed with advantage as a chemical test of electricity, whilst the production of acid and alkali, affected by still inferior degrees of electricity to those required for the coagulation of albumen may likewise be regarded as auxiliary tests on such occasions?

If these facts and observations appear to the Society to throw any light upon the principle of secretion, it may be an advantage to medical science, that they should be laid before the public, as hints for future inquiry.

XXIII. *On the comparative Influence of Male and Female Parents on their Offspring. By* Thomas Andrew Knight, *Esq. F. R. S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R.S.*

Read June 22, 1809.

MY DEAR SIR,

I HAVE been engaged, during many years, in experiments on fruit-trees, of which the object has been to discover the best means of forming new varieties, that may be found better calculated for the climate of Britain than those at present cultivated. In this inquiry my efforts have been always most successful, when I propagated from the males of one variety and the females of another; and I was enabled, by the same means, to ascertain more accurately, than had previously been done, the comparative influence of the male and female parent on the character of the offspring. The analogy that subsists between plants and animals, in almost every thing which respects generation, induced me also to attend very minutely to similar experiments in which I engaged on some species of animals; and as the repetition of such experiments would necessarily require a very considerable space of time, and as the results seem to lead to conclusions that may be of public utility, I have thought the following account sufficiently interesting to induce me to address it to you.

Linnæus conceived, that the character of the male parent predominated in the exterior parts both of plants and animals;

and the same opinions have been generally entertained by more modern naturalists. But the Swedish philosopher appears to have been misled, by the striking predominance of the character of the male parent in male animals, and to have drawn his conclusions somewhat too generally: for I have observed that seedling plants, when propagated from male and female parents of distinct characters and permanent habits, generally, though with some few exceptions, inherit much more of character of the female, than of the male parent, and the same remark is applicable, in some respects, to the animal world, as I shall point out in the succeeding narrative.

My experiments were made on many different species of fruit-trees; but most extensively, and under the most advantageous circumstances, on the apple-tree; and as the results were all in unison with each other, it will be necessary to trouble you only with an account of some of the experiments which were made on that species of fruit-tree.

The apple, or crab of England, and of Siberia, however dissimilar in habit and character, appear to constitute a single species only; in which much variation has been effected by the influence of climate on successive generations: for the two varieties readily bred together, and the offspring, whether raised from the seeds of the Siberian, or British variety, were prolific to a most exuberant extent. But there was a very considerable degree of dissimilarity in the appearance of the offspring; and the leaves, and general habits of each, presented an obvious prevalence of the character of the female parent. The buds of those plants, which had sprung from the seeds of the cultivated apple, did not unfold quite so early in the spring; and their fruits generally exceeded, very con-

siderably, in size those which were produced by the trees which derived their existence from the seeds of the Siberian crab. There was also a prevalence of the character of the female parent in the form of the fruit; but the same degree of prevalence did not extend to the quality and flavour of the fruit; for the richest apple that I have ever seen, and which afforded expressed juice of much higher specific gravity than any other, sprang from a seed of yellow Siberian crab.

The prevalence of the character of the female parent in the preceding cases, may possibly be suspected to have arisen from some error, or neglect of accuracy in making the experiments; but I do not conceive that any such errors could have existed; for the trees of each variety were trained to walls, where they blossomed much before any others of the same species, and the *stamina* were always carefully extracted, whilst immature, from every blossom, which I intended to afford seeds. The remaining blossoms of the trees were also totally destroyed, and no other blossoms, except those from which the pollen was taken, were ever unfolded in the neighbourhood, in the season when the experiments were made; and I have also invariably declined to draw any conclusion from the appearance of a plant, in which I could not certainly distinguish some portion of the features and character of the supposed male parent.

It is perhaps also proper to state, that the predominance of the character of the female parent, could scarcely have arisen from any defective action of the pollen; for, except in cases where superfœtation took place, I have invariably found the effect of a very large, or a very small quantity of pollen, to be invariably the same, in its influence on the offspring; and

in the greater part of the experiments, from which I have drawn the preceding conclusions, more than ten times as much pollen was deposited on the stigmata, as could have been deposited in unmutilated blossoms by the ordinary means employed by nature.

In all attempts to discriminate the different influence of the male and female parent on the offspring of animals many difficulties present themselves, owing to the intermixtures which have been made of the different breeds of domesticated animals of every species, and the consequent absence of all hereditary permanency in the character of each variety. For under these circumstances, the offspring will be very frequently found to shew little resemblance either to its male or female parent, either in form, or stature, or colour. It will therefore be necessary, before I enter on the subject of viviparous animals, to observe that when I apply the terms large and small to the male or female parent, I extend the meaning of those terms to the parentage, from which the male and female descend, and not to the size of the individual only, which becomes the immediate parent of the offspring.

Mr. CLINE has observed, in a communication to the Board of Agriculture, that if the male and female parent differ considerably in size, the dimensions of the fœtus, at the birth, will be regulated much more by the size of the female than of the male parent; and, if the meaning of the terms large and small be extended to the varieties, as well as to the individuals, his remark is perfectly just. But experience compels me wholly to reject the inference that he has drawn respecting the advantages of propagating from large, in preference to small females.

Nature has given to the offspring of many animals (those of the sheep, the cow, and the mare, afford familiar examples) the power, at an early age, to accompany their parents in flight; and the legs of such animals are very nearly of the same length, at the birth, as when they have attained their perfect growth. When the female parent is large, and the fœtus consequently so, the offspring will be large at its birth, in proportion to the bulk it will ultimately attain, and its legs will thence be long comparatively with the depth of the chest and shoulders. When, on the contrary, the female is small, and the fœtus so, at the birth, the length of the legs of the young animal will be short comparatively with the depth of its chest and shoulders; and an animal in the latter form will be greatly preferable, either for the purposes of labour, or of food to mankind. I have seen this difference in the influence of the male and female parent, on the offspring, very strikingly exemplified, in the result of an attempt to obtain very large mules from the male ass and the mare. The largest females, that could be procured, were selected, and the forms of the offspring, at the birth, were perfectly consistent with the theory of Mr. CLINE; they were remarkably large: and I observed, that the length of their legs, when they were only a few days old, very nearly equalled that of the legs of their female parents. I examined the same animals when five years old, and in the depth of their chests and shoulders, they very little exceeded their male parent; and they were consequently of little or no value; whilst other mules, which were obtained from the same male parent (a Spanish ass), but from mares of a smaller size, were perfectly well proportioned. I have never seen a little mule, which is propagated from the

female ass and the horse, nor even a delineation, or description of its form; but I do not entertain any doubt that its chest and shoulders are excessively deep and strong, comparatively with the length of its legs, and that, on account of this peculiarity in its form, it has been so frequently shewn on the Continent, under the name of a jumart, as the pretended off spring of the mare and the bull.

In opposing the theory advanced by Mr. CLINE, it is not by any means my intention to enter the lists with him, as a physiologist; but, as a farmer and breeder of animals of different species, I have probably had many advantages, which he has not possessed; and my conclusions have been drawn from very extensive, and, I believe, accurate observation.

There is another respect in which the powers of the female appear to be prevalent in their influence on the offspring, and that is relative to its sex. In several species of domesticated, or cultivated animal (I believe in all), particular females are found to produce a very large majority, and sometimes all their offspring of the same sex; and I have proved repeatedly, that, by dividing a herd of thirty cows into three equal parts, I could calculate, with confidence, upon a large majority of females from one part, of males from another, and upon nearly an equal number of males and females from the remainder. I frequently endeavoured to change these habits by changing the male; but always without success; and I have in some instances observed the offspring of one sex, though obtained from different males, to exceed those of the other, in the proportion of five or six, and even seven to one. When, on the contrary, I have attended to the numerous offspring of a single bull, or ram, or horse, I have never seen

any considerable difference in the number of offspring of either sex. I am therefore disposed to believe that the sex of the offspring is given by the female parent; and the probability of this seems obvious in fishes, and several other species of animals which breed in water; and though the evidence afforded by the facts adduced is not by any means of sufficient weight to decide the question, it probably much exceeds all that can be placed in the opposite scale.

In oviparous animals, I have had reason to think the influence of the female parent quite as great, as amongst the viviparous tribes, though my observations have been more limited, and less conclusive. In viviparous animals, the size of the foetus is affected by the influence of the male parent, and, in some instances, not inconsiderably; but the size and form of the eggs of birds do not appear to be in any degree changed or modified, by the influence of the male; and therefore the size of the offspring, at the birth, must be regulated wholly by the female parent; and this circumstance permanently affects the form and character of the offspring. The eggs of birds, and those of fishes and insects (if such can properly be called eggs), appear to resemble the seeds of plants, in having their forms and bulk wholly regulated by the female parent; but nevertheless their formation appears to depend on very different laws. For the eggs, both of birds and of fishes and insects, attain their perfect size in total independence of the male, and the cicatricula, the vitellus, and the chalazæ have appeared (I believe) to the most accurate observers, to be as well organised in the unimpregnated, as in the impregnated egg: in the seed, on the _____ every thing relative to its internal organisation _____ the male parent. Spallanzani has,

however, stated, that many plants produced well organised seeds, and even seeds which vegetated perfectly, under circumstances in which it is not easy to conceive how the pollen of the male plant or flower could have been present. But the Italian naturalist appears to have blundered most egregiously in his experiment; or (which I conceive to be more probable) he became the dupe of the refined malice of his countrymen; for, I repeated his experiments under very favourable circumstances, and with the closest attention, but I failed to obtain a single seed. The gourd alone produced apparently perfect fruit, and the seed-coats acquired their natural size and form; and in this respect the growth of its seeds appeared to be, like that of eggs, wholly independent of the influence of the male. But the *seed-coats* of the gourd were perfectly *empty*, and I could not discover, at any period of their growth, the slightest vestige either of cotyledons, or plumule, nor of any thing that appeared to correspond with internal organisation of a seed of the same plant, under different circumstances. SPALLANZANI has not, I believe, mentioned the species of gourd upon which he made his experiments: the common, or orange gourd of our gardens, was the subject of mine.

In comparing the mode of the formation and growth of eggs with the observations I had previously made on the growth of seeds, I have been favoured with the very able assistance of Mr. CARLISLE, for which I have on this, as on many other occasions, to acknowledge much obligation.

I am, my Dear Sir,

with great respect, sincerely yours,

THOMAS AND. KNIGHT.

Downton, May 20, 1809.

XXIV. *On the Effect of westerly Winds in raising the Level of the British Channel. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S. By* James Rennell, *Esq. F. R. S.*

Read June 22, 1809.

DEAR SIR,

IN the " *Observations on a Current that often prevails to the* " *Westward of Scilly,*" which I had the honour to lay before the Royal Society many years ago, I slightly mentioned, as connected with the same subject, the effect of strong westerly winds, in *raising* the level of the British Channel; and the escape of the super-incumbent waters, through the Strait of Dover, into the *then* lower level of the North Sea.

The recent loss of the Britannia East India ship, Captain BIRCH, on the Goodwin Sands, has impressed this fact more strongly on my mind; as I have no doubt that her loss was occasioned by a current, produced by the running off of the accumulated waters; a violent gale from the westward then prevailing. The circumstances under which she was lost, were generally these:

In January last she sailed from her anchorage between Dover and the South Foreland (on her way to Portsmouth), and was soon after assailed by a violent gale between the west and west. The thick weather preventing a view of the

the Goodwin, she struck on the north-eastern extremity of the southernmost of those sands. And this difference between the reckoning (after due allowance being made for the tides) and the actual position, I conclude was owing to the northerly stream of current, which caught the ship when she *drifted* to the *back,* or *eastern side* of the Goodwin.

The fact of the high level of the Channel, during strong winds, between the W. and SW., cannot be doubted: because the increased height of the tides in the southern ports, at such times, is obvious to every discerning eye. Indeed, the form of the upper part of the Channel, in particular, is such as to receive and retain, for a time, the principal part of the water forced in; as may be seen by the sketch (No. 2): and as a part of this water is continually escaping by the Strait of Dover, it will produce a current; which must greatly disturb the reckonings of such ships as navigate the Strait, when thick weather prevents the land, or the lights of the Forelands, and the North Goodwin, from being seen.

I observe in a new publication of Messrs. LAWRIE and WHITTLE, entitled " *Sailing Directions, &c. for the British Chan-* " *nel,* 1808," that throughout the Channel, it is admitted by the experienced persons whom he quotes, that strong SW. winds " cause the flood tide to run an hour, or more, longer, " than at common times:" or in other words, that *a current overcomes the ebb tide, a full hour:* not to mention how much it may accelerate the one, and retard the other, during the remainder of the time.*

---

* It is also asserted, that in the mouth of the Channel, the extraordinary rise of tide, in stormy weather, is 'ten feet: that is, at common springs twenty, and in storms thirty feet. See pages 28, 41, 70, and 133.

It is evident, that the direction of the current under consi-
deration, will be influenced by the form and position of the
opposite shores, at the entrance of the Strait; and as these
are materially different, so must the direction of the stream,
be, within the influence of each side, respectively. For in-
stance, on the English side, the current having taken the di-
rection of the shore, between *Dungeness* and the *South Fore-
land*, will *set* generally to the north-east, through *that* side of
the Strait. (See No. 1.) But, on the French side, circum-
stances must be very different: for the shore of *Boulogne*
trending almost due north, will give the current a like direc-
tion, since it cannot turn sharp round the Point of *Grisnez*,
to the north-eastward; but must preserve a great proportion
of its northerly course, until it mixes with the waters of the
North Sea. And it may be remarked, that the Britannia, when
driven to the eastward of the Goodwin, would fall into this
very line of current.

There is another circumstance to be taken into the account;
which is, that the *shore of Boulogne* presenting a direct obstacle
to the water impelled by the westerly winds, will occasion a
higher level of the sea, there, than elsewhere; and, of course
a stronger line of current towards the Goodwin. (See again
No. 2.)

It must, therefore, be inferred, that a ship, passing the Strait
of Dover, at the back of the Goodwin Sands, during the pre-
valence of strong W. or SW. winds, will be carried many
miles to the northward of her reckoning; and if compelled
to run on it, may be subject to great hazard, from the

It should be remembered, of course, that although the stream

No 2

Portland

Wright

Port mouth

Beachy H

Hastings

Rye

Dover

Margate

Goodwin

Strait of Dover

Calais

Boulogne

Dieppe

Havre

Cherbourg

British Channel

(Sketched from M D knell)

I

* Messrs. LAWRIE and WHITTLE's publication, allows the tides in this quarter a velocity of one mile and a half per hour, at the springs; half a mile at the neaps. The Britannia's accident happened at *dead* neaps.

of current, alone, has been considered here (in order to sim-
plify the subject), yet that, in the application of these remarks,
the regular tides must also be taken into the account. But
from my ignorance of their detail, I can say no more than that
I conceive that the great body of the tide from the Channel,
must be subject to much the same laws, as the current itself.
The opposite tide will doubtless occasion various inflexions
of the current, as it blends itself with it; or may absolutely
suspend it: and the subject can never be perfectly understood,
without a particular attention to the velocity and direction of
the tides in moderate weather, to serve as a ground-work.*

I am, with great respect,

Dear Sir,

your faithful humble servant,

J. RENNELL.

XXV. *On Respiration*. *By* William Allen, *Esq. F. R. S. and* William Hasledine Pepys, *Esq. F. R. S.*

Read June 22, 1809.

ONE of the most prominent features in our last communication was the evolution of a considerable quantity of azote, when oxygen gas nearly pure was respired ; and although a considerable part of this azote must undoubtedly be attributed to the residual gas in the lungs, after the most forcible attempt at expiration, yet the fact seemed to demand still farther investigation, it appearing of consequence to ascertain whether the increase of azote was uniform throughout the latter stages of the experiment, or *solely* confined to the earlier periods.

By adverting to our former Paper, it will be found, that in an experiment where more than 3000 cubic inches of oxygen passed through the lungs in seven minutes and a quarter, 62 cubic inches of azote were found in the first 250 cubic inches expired, though the gas originally contained but 2,5 per cent. or only 6 cubic inches in this quantity ; in the two next portions expired, consisting of 562 cubic inches, we found 56 cubic inches of azote; though this quantity of gas, before it was respired, contained only 14 ; these, first portions, were given off in about five minutes, and contained nearly 100 cubic inches of azote more than could be accounted for in the oxygen respired : hence it is plain, that a large proportion was evolved in the first periods of the process.

Our attention was particularly directed to this point in the following experiment. The oxygen, procured as usual from hyperoxygenised muriate of potash, was found to contain four per cent. of azote; the experiment was conducted in the same manner as the preceding ones, except that the tubes of the gasometers were filled with oxygen, and the gas was not merely passed *once* through the lungs, but breathed backwards and forwards. In order to prolong the duration of the experiment, which began and ended with a forcible expiration, portions of the respired gas were preserved for examination from each of the gasometers, in the following order :

No. 1.   244

2.   294

3.   282

4.   266

5.   230

6.   266

7.   254

8.   288

9.   252

10.   168

2544

The portion of oxygene remaining in the water gasometer of the original quantity, not employed in the experiment, was found upon trial to contain four per cent. of azote, as before.

Summary of the Experiment.

Bar.	Therm.	Cub. Inches of Oxygen inspired.	Cub. Inches of Gas expired.	Deficiency.	Time.
29,9	51	2668	2544	124	13 minutes ;

here the deficiency was greater than we had ever remarked before; but on passing an equal quantity of common air from the water gasometer, and registering it in the mercurial ones, we were satisfied that the apparatus was quite perfect. It is, however, to be considered, that the respiration in this case was not natural, and that some small degree of force was required when the inspirations and expirations were made in the mercurial gasometers, which renders this experiment rather different from those which had preceded it; and it appears to us probable, that a portion of air was forced into the extremities of the bronchea, which could not be suddenly expelled by the strongest attempts at expiration. Hence also, perhaps, the constant though smaller deficiency, even when the air was only once passed through the lungs; but when the process is continued for a much longer time, it is probable that the vessels recover their tone, and are able to expel nearly the whole of the volume admitted.

The air expired in the present instance, being examined in the manner described in our last paper, we found that 100 parts from each of the gasometers contained the following proportions:

No. 1. —— 10 carbonic acid
21 azote
69 oxygen

No. 2. —— 10 carbonic acid
azote
oxygen

No. 3. 10   carbonic acid

8,5   azote

81,5   oxygen

———

100

No. 4. 10   carbonic acid

7,75   azote

82,25 oxygen

———

100

No. 5. 10   carbonic acid

7   azote

83   oxygen

———

100

No. 6 to 10 mixed 10,5   carbonic acid

5,5   azote

84   oxygen

———

100

we shall first calculate the total quantity of azote existing in the gas before the experiment, and afterwards estimate what was produced in the different periods during the first half of the experiment.

Calculation for Azote.

2668 cubic inches of oxygen were employed containing four per cent. azote : then

$$100 : 4 :: 2668 : 106,72$$

the total quantity of azote in the gas consumed, was 106,72 cubic inches.

Azote found after the Experiments.

	Cubic Inches.			Azote found.
No. 1.	244	100 : 21	::	244 : 51,24
2.	294	100 : 11	::	294 : 32,34
3.	282	100 : 8,5	::	282 : 23,97
4.	266	100 : 7,75	::	266 : 20,61
5.	230	100 : 7	::	230 : 16,10
6 to 10.	1228	100 : 5,5	::	1228 : 67,54

Total   211,80 cubic inches.

The whole azote, found after the experi-
ment, was   -   -   -   211,80 cubic inches.

Azote detected by the same tests before
the experiment only   -   -   106,72

       Increase of azote   105,08

Now, as the whole time was thirteen minutes, if we divide
this by the number of gasometers filled, it will give us one
minute eighteen seconds for each, and the following will be
the periods in which the azote was evolved.

	Time.	Azote found.	Azote in the Oxygen.		Increase.
No. 1.	1.18	51,24 less	9,76	equal to	41,48
2.	1.18	32,34 —	11,76	=	20,58
3.	1.18	23,97 —	11,28	=	12,69
4.	1.18	20,61 —	10,64	=	9,97
5.	1.18	16,10 —	9,20	=	6,90
		67,54 —	49,12	=	18,42

Here the increase of azote appears rather greater, *viz.* 11⟨ cubic inches, but the calculation in this case is made upon th⟨ gas *expired*, and, from the above statement, we may see, tha⟨ the evolution of azote goes on diminishing; we have some⟨ times even found, that towards the close of an experiment i⟨ has been almost reduced to nothing. The question now i⟨ whether this increase of azote can be owing to the residu⟨ gas contained in the lungs at the beginning of the experimen⟨ or whether a portion of oxygen is not actually exchanged fo⟨ azote, when pure oxygen gas is respired.

Here it may be useful to compare the azote found in ou⟨ former experiments on oxygen, with the present.

	Bar.	Therm.	Oxygen Gas inspired.	Gas expired.	Deficiency.	Time.	Quantity respired in a Minute.	Azote evolved.	Inferr⟨ Capaci⟨ of Lun⟨
No. 1.		53	3260	3193.	67	9,20	348	110	141
2.	30,3	70	3420	3362	58	7,25	461	177	225
3.	30,15	70	3130	3c60	70	8,45	357	187	236
4.	29,9	51	2668	2544	124	13,	205	105	133

The greatest increase of azote was in the 2d and 3d experiments, when the thermometer was at 70°, which might materially influence the results: in the other cases, it was not higher than 53.

From the experiments of GOODWIN, we might be inclined to admit the capacity of the lungs, inferred from the 1st and 4th experiments, as very possible; but it seems difficult to conceive that it can amount to 236 or 225 cubic inches, and yet this must be the case, unless a portion of azote is given off from the blood, or there is some process in nature by which it is capable of being produced from oxygen.

Having, by the kindness of our friend HENRY CLINE, jun⟨

been furnished with the lungs of a stout man, about five feet ten inches high, taken from the body not long after death, and in a sound state, we proceeded to ascertain the quantity of air contained in this organ after the most complete expiration, as in death.

HENRY CLINE had judiciously taken the precaution to divide the trachea just below the cricoid cartilage, before he opened the thorax; he then inserted a tube with a brass stop-cock, which he tied firmly to the trachea, and attached an empty bladder to the other end. The cock was then turned, so as to communicate with the bladder, and on opening the thorax $31\frac{1}{2}$ cubic inches of air were expelled into it. The weight of the lungs was four pounds one ounce. A very large glass jar being placed in a shallow tin vessel, was filled to the brim with water, the lungs were then completely immersed, and the water which flowed over, and was the measure of their volume, weighed six pounds two ounces; we next cut a portion of the lungs into small pieces, under a large inverted glass of water, and attempted to squeeze the air from the cells, but although several cubic inches were thus procured, we were soon convinced that it was utterly impossible to arrive at our object by these means, as no force that we could use seemed capable of expelling the air from the cellular membrane, into which it escaped from the vesicles. We therefore took portions of the lungs, which weighed 2774 grains; the mass being put into a piece of new hair cloth, was subjected to the force of a powerful screw press, and the fluid was received after twice undergoing this operation, the mass 70 grains. Its specific gravity was very nearly the blood coloured

by the press, was of the specific gravity of 1,019; this would make the specific gravity of the lungs ,997, water being 1,000; hence it appears, that the substance of the lungs, and the contents of the blood-vessels together, are so near the specific gravity of water, that they may be fairly considered as the same.

Then, as the mass of the lungs was equal to 4 pounds of water, though 6,2 pounds of water were displaced by them, and as a pound of water occupies the space of 28,875 cubic inches, we have the following calculation:

lbs. oz.
6 2 water displaced by the lungs
4 1 weight of the lungs
———
2 1, or 59,554 cubic inches of air in the lungs, to which must be added 31,580 the volume of the air forced into the
——— bladder on opening the thorax.
91,134
———

and this gives us 91,134 cubic inches, as the air contained in the lungs of this person after death; and when we reflect that the air must have been under compression, when the lungs were immersed in water, some force being required to keep them down, and also that not less than 7 or 8 cubic inches must be contained in fauces, &c., we cannot estimate the whole at less than 100 cubic inches.

It is farther to be noted, that these 100 cubic inches would occupy much more space in the temperature of the human body, than in the mean temperature in which the examination was made; and this difference would be nearly 8 cubic inches; the air left in the lungs, after complete expiration, would

therefore be 108 cubic inches; but the mean of our experiments would make it 183.

$$
\begin{array}{rr}
\text{Experiment 1.} & 141 \\
\text{2.} & 225 \\
\text{3.} & 236 \\
\text{4.} & 133 \\
\hline
4)735 \\
\hline
183
\end{array}
$$

we are then almost compelled to allow that when pure oxygen is respired, a portion of azote is given off from the blood.

We now resolved to perform a series of experiments upon some animal which lived wholly upon vegetable food, and made choice of the Guinea pig as one of the most manageable.

The apparatus consisted of our two large mercurial gasometers, which were made to communicate with a strong trough E, in the middle of which a small mahogany table D was made fast by a screw, for the purpose of supporting the animal under the bell-glass A, two holes were made through the table for the insertion of tubes to supply, and take off the air, each of them communicated with one of the mercurial gasometers; the tube B delivered gas towards the upper part of the glass A, in order to bring the supply of fresh air near the head of the animal: the opening of the tube C was placed within half an inch of the table to convey off the respired air; the gasometer connected with this tube, was made to communicate with a mercurial bath G, in which portions of the respired air were preserved for examination. Quicksilver being

poured into the trough E, so as to rise to a level with the top of the mahogany stand, we placed a Guinea pig upon it, with the bell-glass over him, and as its edges were immersed in quicksilver, the animal was completely confined in atmospheric air: we found that his body occupied the space of 39 cubic inches, which deducted from the cubic contents of the glass A, left 55 cubic inches for the air confined with the pig, to which must be added 5 more for that contained in the tube C.

First Experiment with Atmospheric Air.

The pig was placed upon the stand, and the apparatus arranged as represented in the plate: 250 cubic inches of atmospheric air were admitted into the mercurial gasometer communicating with B: the gasometer communicating with C was quite empty, the apparatus being tried was found perfectly air tight, and the whole quantity of air 310 cubic inches.

The cocks H and I being opened, gentle pressure was made upon the glass of gasometer B, so as to cause the air to pass through A, which consequently drove an equal portion through the tube C into the empty gasometer; a quarter of an hour was employed in passing the gas, which measured exactly 250 cubic inches in C, so that there was no alteration of volume; the cocks H and I were now closed, and the respired air being examined by the usual methods, 100 parts were found to contain

> 5 carbonic acid
> 16 oxygen
> ·79 azote.

> 100

As the air after the experiment had experienced no alteration of volume, and as it contained the same proportion of azote as atmospheric air, this substance had remained unaltered. But 15,50 cubic inches of oxygen had been converted into carbonic acid gas.

$$100 : 5 :: 310 : 15,50.$$

Summary of the Experiment.

Bar.	Therm.	Atmos. air Inspired.	Gas after experiment.	Cub. inches of carb. acid.	Cub. in. of carb. acid per minute.	Time.
30°	43°	310	310	15,5	,62	25 min.

Experiment II. Atmospheric Air.

The experiment was repeated in exactly the same manner; the animal, except from confinement, appeared much at his ease all the time. The air after the experiment contained in 100 parts

<div style="text-align:center">

5,5 carbonic acid

15,5 oxygen

79 azote

——

100

</div>

here, the proportions of azote were undisturbed, and 17,05 cubic inches of carbonic acid procured

$$100 : 5,5 :: 310 :: 17,05$$

Summary of the Experiment.

Bar.	Therm.	Atmos. air Inspired.	Air after Experiment.	Carb. acid found.	Carb. acid per minute.	Time.
56	38°	310	310	17,05	,68	25 min.

Experiment III. Atmospheric Air.

cubic inches of atmospheric air through it, which measured 1001 : portions of the respired gas had been preserved in the mercurial bath, and the usual trials made upon the mixture, which was found to contain 5 parts of carbonic acid in every 100, or 53 cubic inches in the whole quantity; the azote was unaltered; 100 : 5 : : 1060 : 53.

Summary of the Experiment.

Barom.	Therm.	Atmos. air before expt.	Air after expt.	Increase.	Carb. acid. found.	Carb. acid per minute.	Time.
29,8	56°	1060 .	1061	1	53	,88	1 hr.

Experiment IV. *Oxygen Gas.*

The pig hitherto employed was put into the glass vessel A, which with the tube contained 60 cubic inches of atmospheric air; 250 cubic inches of oxygen, containing 5 per cent. of azote, were admitted into the gasometer communicating with B, and during a quarter of an hour were made to pass slowly through the vessel in which the animal was confined, to the empty gasometer communicating with C, where it measured exactly 250 cubic inches; a portion was preserved in the mercurial bath for examination, and the gasometer B was replenished with 250 cubic inches of the same oxygen; this was passed in about the same time as before, through A into gasometer C, when it measured 248 cubic inches.

250 cubic inches more of the oxygen were now admitted into gasometer B, and passed in the same manner through A into C, where they measured 249.

The gasometer B was for the fourth and last time supplied with 250 cubic inches more of the oxygen, which were passed as before, through A into C, during about a quarter of an hour, and then measured 249.

The pig had remained in the vessel one hour and twelve minutes; it did not appear to have suffered in the least; portions of the respired gas were saved from each of the gasometers, and examined as usual.

	Cubic Inches.	Contained in 100 parts.		Carb. Acid.	Oxygen.	Azote.
No. 1.	250	Carb. acid	8	20		
		Oxygen	66		165	
		Azote	26			65
			100			
No. 2.	248	Carb. acid	10	24,80		
		Oxygen	78		193,44	
		Azote	12			29,76
			100			
No. 3.	249	Carb. acid	10	24,90		
		Oxygen	80		199,20	
		Azote	10			24,90
			100			
No. 4.	249	Carb. acid	12	29,88		
		Oxygen	79		196,71	
		Azote	9			22,41
			100			
In Glass A, and tube C.	60	Carb. acid	12	7,20		
		Oxygen	79		47,40	
		Azote	9			5,45
	1056		100	106,78	801,75	147,52

Total, gas before experiment,   1060
      after   1056

Deficiency,   4

Calculation for Oxygen.

We do not calculate upon the tube from gasometer B, because it is always in the same state after the experiment as before.

1000 cubic inches of oxygen containing
5 per cent. azote, consisted of .... 950 pure oxygen

60 Atmospheric air with the pig, and in
tube C, containing 21 per cent. oxygen ... 12,60

Total, oxygen before experiment, 962,60
Oxygen found after experiment, 801,75
Ditto in carbonic acid   -   106,78

908,53

Oxygen missing, 54,07

Calculation for Azote.

1000 cubic inches containing 5 per
cent. azote   -   -   -   50

60 Atmospheric air, containing 79 per cent. 47,40

Total azote before experiment, 97,40
Ditto found after experiment, 147,52

Increase of azote, 50,12

This increase of azote was much more than equal to the cubic contents of the animal's body, the deficiency of 4 cubic inches was doubtless oxygen absorbed.

Summary of the Experiment.

Bar.	Therm.	Oxygen, &c. inspired.	Gas after experiment.	Defi- ciency.	Carb. acid found.	Carb. acid per mm.	Time.	Oxygen missing.	Azote added.
29,05	57°	1060	1056	4	106	1,48	1 h. 12 m.	54,07	50,12

Experiment V.　*Oxygen.*

In this experiment we employed a smaller pig, which occupied the space of 33 cubic inches, and our object was to keep him for the same length of time in a smaller quantity of gas, we therefore only used 750 cubic inches of oxygen, beside the common air contained in the glass A, and tube, amounting to 66 cubic inches ; the first 250 cubic inches were passed through the glass A into C in 24 minutes, where it appeared to have undergone no alteration of volume. 250 cubic inches more were passed during the next 23 minutes, and these measured 248 in C ; the last 250 were passed in 24 minutes, and the volume remained unaltered. The animal did not appear to suffer the slightest inconvenience, except from the confinement.

State of the Gas before Respiration.

		Oxygen.	Azote.
66 cubic inches of atmospheric air,	=	13,86	52,14
750 oxygen, containing 5 per cent. azote,	=	712,50	37,50
816	total consisting of	726,36	89,64

The oxygen was tried before, as well as after the experiment, and both the results agreed perfectly with each other. We now examined portions of gas preserved from the three gasometers, with lime water, and the tests for oxygen.

	Time. min.	Contained in 100 parts.		Carbonic acid.	Oxygen.	Azote.
No. 1. 250.	24	Carb. acid	9,5	23,75		
		Oxygen,	60,5		151,25	
		Azote,	30			75
			100			
No. 2. 248.	23	Carb. acid,	9,5	23,56		
		Oxygen,	81		200,88	
		Azote,	9,5			23,56
			100			
No. 3. 250.	24	Carb. acid,	10	25		
		Oxygen,	82		205	
		Azote	8			20
			100			
66 with pig, as No. 3.				6,60	54,12	5,28
814	71			78,91	611,25	123,84

Calculation for Oxygen.

Oxygen before the experiment   -   726,36
Ditto after - - - 611,25
In carbonic acid -   - 78,91
         690,16

Loss of oxygen 36,20

Calculation for Azote.

Azote found after experiment - - 123,84 .
Ditto before experiment - - 89,64

Increase of azote 34.20

Summary of the Experiment.

Therm.	Gas before Exper.	Gas after Exper.	Defici- ciency.	Carbonic Acid found	Cubic Inches per Minute.	Time. h ʺ	Oxygen missing	Azot added.
56°	816	814	2	78,91	1,11	1 11	36,20	34,20

The quantity of azote added, of oxygen missing, and of carbonic acid formed, were smaller than in the last experiment; but the animal in this instance was smaller, as well as the quantity of oxygen passed through in a given time.

In this case, as in the human subject, the increase of azote takes place principally in the early periods. The whole azote contained in the 66 cubic inches, confined with the pig, was only 52,14, but supposing, which perhaps was not the case, that the 66 of common air were expelled by the first 250 cubic inches of oxygen, we should have 250

less   66
───────
184 of oxygen,

containing 5 per cent. azote, or 9,20 cubic inches; these added to the 52,14, would make 61,34 of azote to be found in the first gasometer of respired gas, but we detected 75, so that even on this supposition 13,66 of azote were added in the first twenty-four minutes.

The azote contained in the second gasometer before respiration, was 12,50 cubic inches, but after it had been respired for twenty-three minutes, we found 23,75, or an increase of 11,25 azote. The azote contained in the third gasometer, before respiration, was, as before, 12,50 cubic inches; but after it had been respired for twenty-four minutes, we found 20, or an increase of 7,50 azote.

The azote contained in the 66 cubic inches, was 5,80, but we found the whole increase of the azote.

From the results of these experiments, it seemed that when the *usual proportion* of azote was not present in the gas respired, there was a disposition in the blood to give out a certain quantity in exchange for an equal volume of oxygen, and we resolved to try, whether this circumstance would occur when hydrogen was substituted for azote, we accordingly made a mixture containing 22 per cent. oxygen and 78 hydrogen.

Experiment 6. Hydrogen and Oxygen.

The pig employed in the last experiment, was placed upon the stand in the glass A, with 66 cubic inches of common air as usual.

250 cubic inches of the mixture were passed from the gasometer, communicating with B through the glass A into the gasometer communicating with C during sixteen minutes. The animal did not appear uneasy: a second quantity of 250 cubic inches was passed in seventeen minutes and three quarters: the animal did not seem to be in the least incommoded.

A third quantity of 250 cubic inches was passed, in about sixteen minutes.

And a fourth quantity of 250 cubic inches in eleven minutes and three quarters; but during this time, the animal became very sleepy, and towards the end of the experiment, kept his eyes constantly shut; he, however, appeared to suffer nothing, and was easily roused for a short time by rapping at the side of the glass. At the end of sixty one minutes and a half, he was taken out, and we found that during this time, he had produced 60,20 cubic inches of carbonic acid gas, or rather less than one cubic inch in a minute.

It appears, that less carbonic acid was evolved in this instance in a given time, than when oxygen was respired, but

some circumstances occurred to prevent us from discovering what change the azote had experienced: this point was, however, decided by the following experiment.

Experiment 7. *Hydrogen and Oxygen.*

Having mixed hydrogen and oxygen gases in such proportion as that the oxygen should rather exceed the quantity contained in atmospheric air, we placed the same animal in the glass A with 66 cubic inches of atmospheric air, 250 cubic inches of the mixture were admitted into gasometer B, from the large water gasometer, and gradually passed through the glass A into gasometer C, during fifteen minutes. The pig did not appear uneasy, and the respired gas measured 250 in C: a portion of this was preserved for examination, which we shall call No. 1.

250 cubic inches more of the mixture were admitted into B, and gradually passed, as before, during thirteen minutes; it measured 250 in C; and a portion No. 2 was preserved for examination.

The animal did not seem to suffer any inconvenience, 250 cubic inches more of the mixture were admitted into B, and gradually passed, as before, through A into C during seventeen minutes. The animal now become quite sleepy, but did not appear to suffer any thing. He was taken out at the end of forty minutes.

At the close of the experiment, the remains of the mixture, which had stood about an hour in the large water gasometer, examined was found to contain 22 per cent. of oxygen and of the residual 78 parts, 20 were mixed with previously found to contain

3 per cent. azote; these 30 parts being detonated in DAVY's improved VOLTA's eudiometer, by the electric spark, were reduced to 3 parts, and these 3 parts being treated with the tests for oxygen, were reduced to 2 parts, *a proof that all the hydrogen had been consumed;* but the 10 parts of oxygen contained, 3 of azote; this deducted from 2, leaves 1,7 for the azote contained in 20 parts of the residuum 78.

$$20 : 1,7 :: 78 : 6,6$$

The mixture employed, therefore, contained in every 100 parts.

$$
\begin{array}{l}
22 \quad \text{oxygen} \\
6,6 \text{ azote} \\
\underline{71,4} \text{ hydrogen} \\
100
\end{array}
$$

We next examined the gas which had been respired,

No. 1. 250 cubic inches respired during fifteen minutes.

100 parts, subjected to the action of lime water in PEPYS's eudiometer, were reduced to 93,5, and this by the tests for oxygen was farther diminished to 77: 20 parts of this 77, mixed with 10 of oxygen and detonated, the residuum treated with the tests for oxygen, left 12 parts which were azote,

From these 12 parts

Deduct ,3 for the azote in the 10 parts oxygen

Leaves $\overline{11,7}$ for the azote contained in 20 parts of the residual 77.

$$20 : 11,7 :: 77 : 45$$

No. 1, therefore consisted in 100 parts of

$$
\begin{array}{l}
6,5 \text{ carbonic acid} \\
16,5 \text{ oxygen} \\
45 \quad \text{azote} \\
\underline{32} \quad \text{hydrogen} \\
100
\end{array}
$$

No. 2. 250. Respired during thirteen minutes; 100 parts were reduced by lime water to 92,5, and these by the tests for oxygen to 77. Of these 77 parts, 20 being mixed with 10 of oxygen, and detonated, were diminished to 4, and these 4 being examined for oxygen left 3, which must be azote:

> From these   3
> Deduct   ,3   for azote in the 10 parts oxygen,
> ─────
> Leaves   2,7   for the azote contained in 20

parts of the residual   77.

$$20 : 2,7 :: 77 : 10,4$$

No. 2. therefore consisted in 100 parts, of

> 7,5 carbonic acid,
> 15,5 oxygen,
> 10,4 azote,
> 66,6 hydrogen,
> ─────
> 100

No. 3. 250. respired during seventeen minutes; examined as above, consisted in 100 parts, of

> 6   carbonic acid,
> 17   oxygen,
> 6,5 azote,
> 70,5 hydrogen,
> ─────
> 100

the 66 remaining with the animal at the close of the experiment, may be considered as very nearly the same as No. 3. In this, as in the former experiment, we observed that the emission of carbonic acid was greatest at the middle of the process, and considerably diminished toward the end, as the animal became exhausted; we presume, therefore, that dur-

ing sleep, less carbonic acid is evolved than when the animal is exercising all its faculties.

When atmospheric air alone is respired, we have uniformly found, that the carbonic acid evolved, added to the oxygen remaining, exactly equalled the oxygen existing in the air before it was respired, but in the present instance it was one per cent. more, a circumstance which we are at present unable to account for, but it was constantly the case in all the three trials.

Calculation for Azote.

From the foregoing statement we are enabled to ascertain the quantities of azote, both before and after the experiment.

Azote before the Experiment.

66 cub. inches atmospheric air, with the animal con-

tained $\frac{79}{100}$ or   -   -   -   52,14

750 ———————— of the mixed gasses contained $\frac{6,6}{100}$ or   49,50

816 total gas employed   101,64

The total azote before the experiment was therefore 101,64 cubic inches.

Azote after the Experiment.

		Respired during.					
No. 1.	250.	15 min.	100 : 45	: : 250 :	112,50
2.	250.	13 min.	100 : 10,4	: : 250 :	26
3.	250.	17 min.	100 : 6,5	: : 250 :	16,25
	66.		100 : 6,5	: : 66 :	4,29

          816   45 min.   Azote after experiment   159,04

                             Ditto before   101,64

                             Increase of azote   57,40

Calculation for Hydrogen.

Hydrogen before Experiment.

The mixture before the experiment was found to contain 71,4 hydrogen.

$$100 : 71,4 : : 750 : 535,50$$

therefore the total quantity must be 535,50 cubic inches.

Hydrogen after Experiment.

No. 1. 250	100 : 32	: : 250 :	80
2. 250	100 : 66,6	: : 250 :	166,50
3. 250	100 : 70,5	: : 250 :	176,25
66 in A	100 : 70,5	: : 66 :	46,53

Hydrogen found after experiment	469,28

Hydrogen before the experiment	535,50
Ditto after - - -	469,28

Loss of hydrogen, - -	66,22

In this experiment, as well as in those with oxygen, the proportion of azote evolved, was greater in the early than in the later periods, and it becomes interesting to contrast them: thus we know that 52,14 cubic inches of azote were in the vessel with the animal at the beginning of the experiment, and that, of the 250 cubic inches of mixed gases passed in the first fifteen minutes, only 184 could be expelled into gasometer C, (100 : 6,6 : : 184 : 12,14) which

contained itself   12,14

making together   64,28 of azote, which was all that

could have been expected in the first gasometer of 250 after respiration, supposing the *whole* of the common air had been expelled, but we detected 112,50, or an increase of 48,22 cubic inches in fifteen minutes.

The second gasometer before it was connected with the glass A, contained but 16,50 cubic inches of azote; we found however about 26, and what is remarkable, in the last gasometer there was no increase at all.

Calculation for Carbonic Acid.

No. 1. 250. 15 min. 100 : 6,5 : : 250 : 16,25
2. 250. 13 min. 100 : 7,5 : : 250 : 18,75
3. 250. 17 min. 100 : 6 : : 250 : 15
66. - 100 : 6 : . 66 : 3,96
_____ _____
45 53,96

The quantity of carbonic acid evolved in 45 minutes was therefore 53,96 cubic inches, or at the rate of 1,19 cubic inches per minute.

The foregoing experiments seem to prove,

1. That when atmospheric air alone is respired, even by an animal subsisting wholly upon vegetables, no other change takes place in it, than the substitution of a certain portion of carbonic acid gas, for an equal volume of oxygen.

2. That when nearly pure oxygen gas is respired, a portion of it is missing at the end of the experiment, and its place supplied by a corresponding quantity of azote; the portion evolved in a given time, being greater in the early than in the later periods.

3. That the same thing takes place when an animal is made to breathe a mixture of hydrogen and oxygen, in which

the former is in nearly the same proportion to the latter, as azote to oxygen in atmospheric air.

4. That an animal is capable of breathing a mixture of 78 parts hydrogen, and 22 oxygen for more than an hour, without suffering any apparent inconvenience.

5. That the excitability of an animal is much diminished when he breathes any considerable proportion of hydrogen gas, or that it at least has a tendency to produce sleep.

6. That there is reason to presume an animal evolves less carbonic acid gas during its sleeping, than in its waking hours.

7. That the lungs of a middle sized man contain more than 100 cubic inches of air after death.

These experiments have been conducted without reference to any particular theory, and indeed some of the results were so contrary to our preconceived opinions, that we have been induced to bestow more than ordinary attention on the subject. Confident, however, that all those who repeat the experiments with the same care will arrive at the same results, we shall rest satisfied with stating the facts, not without a hope that those brilliant discoveries of Professor DAVY, which have already given us new views of the operations of nature, will in their progress furnish us with that explanation which it is in vain to expect at present.

Azote or nitrogen, for instance, has been considered as a simple or elementary substance; it is recognised, however, principally by its negative properties. Every gaseous fluid which does not support life or combustion, which is not acted upon by the tests for oxygen, nor
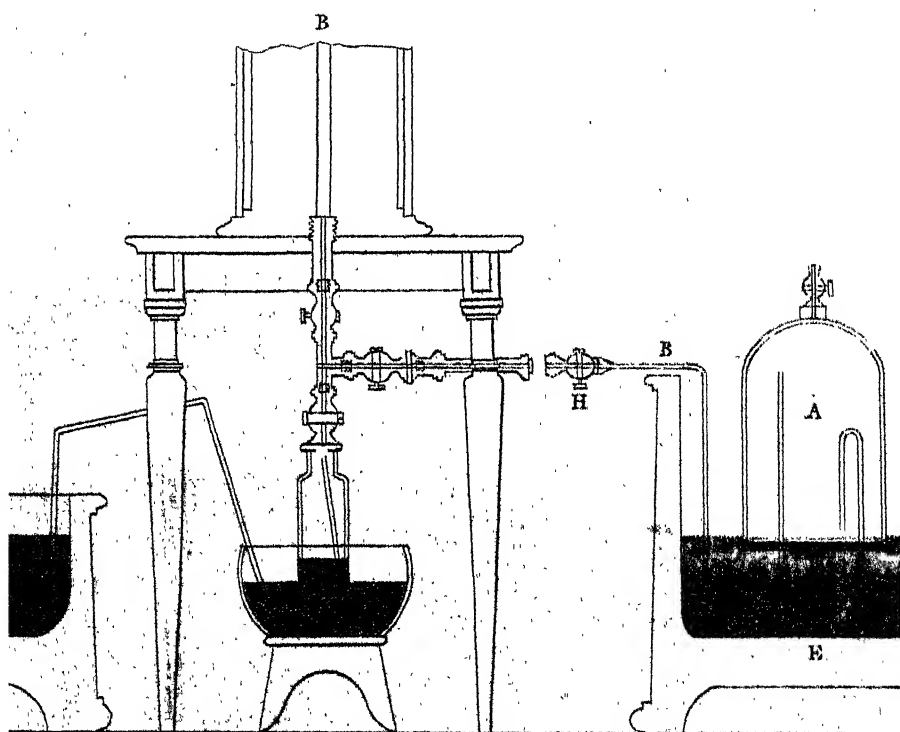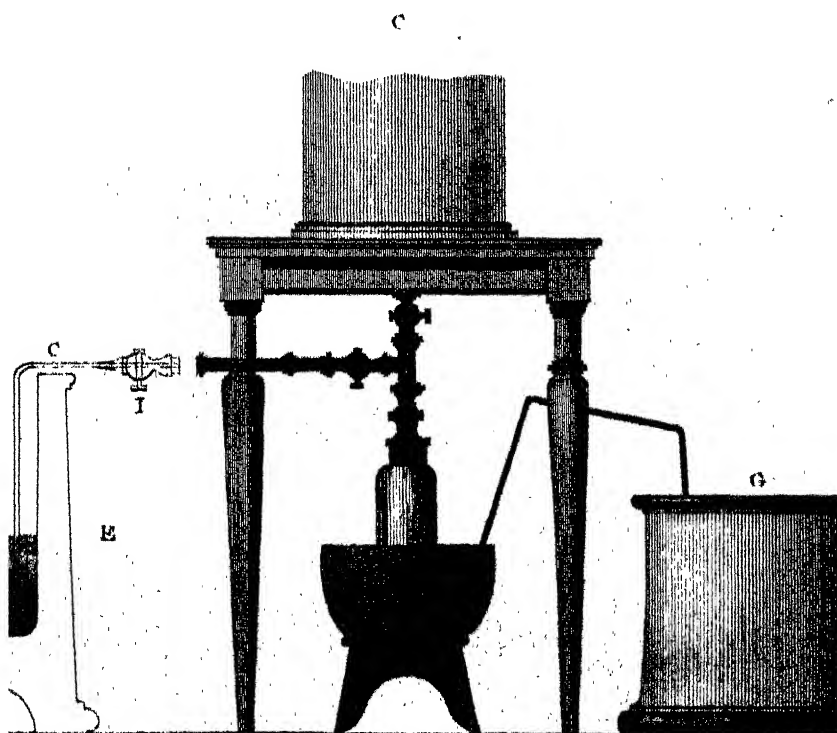
nounced to be azote : it is the constant residuum in almost all our experiments upon gases, but who shall say whether this residuum is a simple substance or a compound ?

The experiment of Professor BERZELIUS, leads us to suspect it of metallic properties ; and those of DAVY make it probable that it is an oxydated body ; the subject is still under discussion. But we may fairly indulge more than a hope that the ardent zeal, and well directed labours of the philosophers just mentioned, will throw a new and important light upon this obscure and difficult subject.

XXVI. *Experiments on Ammonia, and an Account of a new Method of Analyzing it, by Combustion with Oxygen and other Gases; in a Letter to* Humphry Davy, *Esq. Sec. R. S. &c. from* William Henry, *M. D., F. R. S. V. P. of the Lit. and Phil. Society, and Physician to the Infirmary, at Manchester.*[*]

MY DEAR SIR,

I SHOULD sooner have communicated the account, which you are so good as to request, of my further experiments on the decomposition of ammonia, if I had not been anxious to obtain, by frequent and careful repetition of them, results not affected by any of those numerous causes of error, which easily insinuate themselves into processes of so much delicacy. You have already been informed that the fact, which I lately mentioned to you, (tending to prove the existence of oxygen as an element of the volatile alkali, by the discovery of oxygen gas in the products of its analysis) is not entitled to confidence, owing to the admission of a small quantity of atmospherical air, in a way which was not at all suspected. Frequent repetitions of the same process, under circumstances wholly unobjectionable, have fully satisfied me, that no portion whatsoever of oxygen gas is evolved by electricity from ammonia even when, by means of an apparatus constructed for the

purpose, the only metallic surface, exposed to the gas, consists of the sections of two platina wires, each $\frac{1}{50}$ of an inch in diameter, the wires themselves being inclosed in glass tubes which are sealed hermetically round them, and then ground away, so as to expose only the points. Nor does any difference in the nature of the products arise from electrifying the gas either under increased or diminished pressure, the latter of which, it appeared to me probable, from the known influence of elasticity in impeding the combination of gaseous bases, might prevent the oxygen of the alkali from uniting with hydrogen to form water, and occasion the expansion of both into the state of gas.

Having failed, therefore, to acquire, in this way, proof of the existence of oxygen in the volatile alkali, I was next led to seek for some unequivocal mode of evincing the production of water by the same operation; a fact, which would be scarcely less satisfactory in establishing oxygen to be one of its constituents, than the actual separation of oxygen gas. The most careful observation of ammonia, during and after the agency of electricity, does not discover the smallest perceptible quantity of moisture. In order, therefore, to subject the gas to a satisfactory test, I had recourse to the following contrivance. Ammoniacal gas, I had previously found, may be so far desiccated by exposure to caustic potash, as to shew no traces of condensed moisture, on the inner surface of a thin glass vessel containing it, when exposed to a cold of 0° Fahrenheit; though the recent gas, by the same treatment, is made to deposit water in the state of a thin film of ice. A glass globe, of the capacity of between two and three cubical inches, was filled with gaseous ammonia, which was then

dried by sticks of pure potash, fastened to pieces of steel wire, so that they could be withdrawn, after having exerted their full action.   This point of dryness was ascertained by applying æther, or a mixture of snow and salt, to the outside of the globe.   By means of a peculiar apparatus, the gas was next strongly electrified, and the cooling power was again applied to the outer surface of the globe.

In the first trials, that were made with this apparatus, water certainly seemed to have been formed by the electrization of the alkaline gas; for the same portion of gas, which was not affected by a freezing mixture before the process, gave evident signs of condensed moisture, when the cooling power was applied after long continued electrization.   The appearance was not only quite satisfactory to myself, but to Mr. DALTON, and several other chemical friends, to whom I shewed the experiment.   Finding, however, that the appearance varied as to its degree, I was induced to repeat the process with redoubled precaution; filling the globe, previously heated, with hot mercury, and drying not only the quicksilver, but the iron cistern which contained it, by exposure to long continued heat.   The electrified gas now betrayed no signs of moisture on the application of a temperature 20° of FAHRENHEIT; and gave only the smallest perceptible traces, by a cold of 0° or a few degrees below.   I cannot help suspecting, therefore, that the moisture, manifested in the earlier experiments, was derived from the mercury or from some extraneous source, and was not generated by the action of elec-

The avidity with which ammonia retains moisture, and again absorbs it when artificially dried, is very remarkable. A confined quantity of common air may be completely desiccated, in the space of a few minutes, by pure potash, or by muriate of lime; so that no ice shall appear in the inner surface of the containing vessel, when exposed to a cold of — 26° of FAHRENHEIT. But ammonia requires exposure during some hours to potash, to stand the test even of 0° FAHRENHEIT; and a single transfer of the dried gas, through the mercury of a trough in ordinary use, again communicates moisture to it. Muriatic acid gas, freed merely from visible moisture, deposits no water at the temperature of 26° FAHRENHEIT. This is probably owing to its strong affinity for water; for electricity, after the full action of muriate of lime, evolves, as I have lately ascertained, about $\frac{1}{35}$th its bulk of hydrogen gas, the recent muriatic acid gas giving about $\frac{1}{14}$th after the same treatment.*

have been generated by electricity. But though this supposition may explain the non-appearance of *visible moisture*, it does not account for the inefficiency of a powerful cooling cause to discover traces of watery vapour: for this is a test which renders apparent very minute quantities of water in gases.

* In a course of experiments, which I have described in the Philosophical Transactions for 1800, it appeared that muriatic acid gas, after being dried by muriate of lime, gave nearly as much hydrogen by electrization, as gas which had not been thus exposed. I was not however aware, at that time, of the extreme caution necessary in experiments of this kind; and was satisfied with transferring the acid gas from a large vessel, in which it had been dried, into the electrizing tube, a mode of proceeding which I now find to be quite inadmissible. The action of muriate of lime, which has undergone fusion, on muriatic acid gas, is rendered very sensible, when considerable quantities are used, by the evolution of much heat, and by a diminution of the volume of the gas. Ammonia, also, is contracted in bulk by dry caustic potash. Muriate of lime cannot be employed for its desiccation, since this substance rapidly absorbs

From the average of a great number of experiments on the decomposition of ammonia by electricity, I was for some time led to believe, that you had rather under-stated the proportion of permanent gases obtainable from it by this process, (viz. 108 measures of permanent gas, from 60 of ammonia or 180 from 100). For the most part, I had found the bulk of ammonia to be doubled by decomposition, even when the gas was previously dried with extreme care. In one instance, a small bit of dry potash was left in the tube, along with the ammonia, during electrization, with the view of its absorbing water, which I supposed, at that time, to be generated by the process. In this case, 59 measures (each = 10 grains of mercury) became 115. The following table shews the expansion of various quantities of ammonia.

Exp.

1.	60 measures of ammonia, gave permanent gas		112
2.	60	- - - - -	120
3.	59 (potash being left in the tube)	- -	115
4.	55	- - - - -	115
5.	75 (under the pressure of half an atmosphere)		150
6.	65	- - - - -	130
7.	65 .	- - - - -	130
8.	53 (one of the conductors being of steel wire)		106
	492		978

and 492 : 978 : : 100 : 198,78. These proportions, you will find, correspond very nearly with those long ago stated

the same gas, even when the gas has been previously exposed to quick-lime. In this case, separating a portion of muriatic acid from the earth, agreeably to the observations long ago illustrated by BERTHOLLET.

by BERTHOLLET,* who converted 17 measures of ammonia by electrization, into 33 measures of permanent gas, which is at the rate of 194 from 100. Having lately, however, carried on the process with the observance of additional precaution, (the mercury being first boiled in the tube, before admitting the ammonia, and still remaining hot when the gas was passed up), I have obtained from the alkali less than double its volume of permanent gas, viz. 280 measures from 155, or at the rate of 180,6 from 100. The variableness of the first set of results arises, I believe, from the uncertainty of the quantity of ammonia decomposed. For if the smallest portion of moisture remain in the tube, a little ammoniacal gas will be absorbed, and will be slowly given out again as the electrization goes on, thus rendering the actual quantity submitted to experiment greater than appears. It is probable, also, from a fact which I shall afterwards state, that mercury itself, unless when heated, may absorb a small portion of alkaline gas.

The proportion of the hydrogen and nitrogen gases to each other in the products of ammonia decomposed by electricity, I am satisfied, by recent experiments (June, 1809) is as nearly as possible what you have determined, viz. 74 measures of hydrogen gas to 26 of nitrogen. The nearest approximation I have made to these numbers is 73,75 to 26,25. Our only method of analyzing mixtures of these two gases, (viz. by combustion with a redundancy of oxygen) is not, I believe, sufficiently perfect to afford a nearer coincidence.

The extreme labour and tediousness of the decomposition of ammonia by electricity, influenced me to attempt the discovery of a shorter and more summary method of analysis. The

* Journal de Physique, 1786, ii. 176.

most obvious one, was its decomposition by oxymuriatic acid gas; but this plan was abandoned, from the impossibility of confining both the gases by any one fluid; since water acts powerfully on the one, and mercury on the other. But a mixture of oxygen and ammoniacal gases more than answered my expectations. When mingled in proper proportions, these gases, I have ascertained, may be detonated over mercury by an electric spark; exactly like a mixture of vital and inflammable air; and the results of the process, with due attention to the circumstances, which will soon be stated, afford an easy and precise method of analyzing, in the space of a few minutes, considerable quantities of the volatile alkali. With a greater proportion of pure oxygen gas* to ammonia than that of three to one, or of ammonia to oxygen than that of three to 1.4, the mixture ceases to be combustible. When the proportions best adapted to inflammation are used, oxygen gas may be diluted with six times its bulk of atmospherical air, without losing its property of burning ammonia.

Atmospherical air alone does not, however, inflame with ammonia, in any proportion that I have yet tried; though, by long continued electrization with air, ammonia is at length decomposed; its hydrogen uniting with the oxygen of the air and forming water, while the nitrogen of both composes a permanent residuum. Forty-five measures of ammonia being electrified with eighty-six of common air, the total 181 became 196, and 152 after being washed with water. Of 17.2 measures of oxygen contained in the 86 measures of air at ... were left, and these, also, would probably ... continuing the operation after ...

ture of ammonia and atmospheric air, each previously dried by caustic potash and then electrified, be examined, the production of water is made sufficiently apparent on applying ether to the containing vessel. In subjecting ammonia, therefore, to this test of the generation of water by electricity, the purity of the gas from atmospheric air should be carefully determined.*

The products of the combustion of ammonia with oxygen vary essentially, according to the proportion of the gases which are employed. If the oxygen gas exceed considerably the ammonia (that is, if its volume be double or upwards) the ammonia entirely disappears; and no gases remain, but a mixture of nitrogen with the redundant oxygen. The moment the detonation is completed, a dense cloud appears,† and soon afterwards settles into a white incrustation on the inner surface of the tube. The quantity of this substance, which is produced, is too minute for analysis; but its characters resemble those of nitrate of ammonia, the acid ingredient of which is probably generated by the action of oxygen on the nitrogen of one part of the volatile alkali. Accordingly, when the excess of oxygen is removed by sulphuret of lime,

* The result of this experiment shews, moreover, that even supposing oxygen to be a constituent of ammonia, we are not to expect its evolution, in a separate form, by electricity; since, when electrified with ammoniacal gas, oxygen gas is deprived of its elastic form, and its base is condensed into water, by union with nascent hydrogen evolved from the alkali.

† In some cases I have observed, that when the cloud does not occur immediately, it may be made to appear by agitating the quicksilver contained in the detonating tube. This is probably owing to the disengagement of some ammonia, which had lodged in the mercury. The fact confirms what I have already suggested, respecting the cause of the variable proportion of gases, evolved from ammonia by electricity.

the nitrogen generally falls short of the proportion, which ought to accrue from a given weight of ammonia; and hence it is scarcely possible to attain, when a considerable excess of oxygen is used, an accurate analysis of the volatile alkali.

When, on the contrary, the ammonia exceeds considerably the oxygen gas, no production of nitrous acid appears to take place; for the residue, after detonation, is quite free from cloudiness. It is remarkable, however, that ammonia when fired, in certain proportions, with less oxygen than is required to saturate its combustible ingredient, is nevertheless completely decomposed. Part of its hydrogen is sufficient for the saturation of the oxygen; and the remaining hydrogen, and the whole nitrogen of the ammonia, together with that existing as an impurity in the oxygen employed, remain in a gaseous state, and compose a mixture, which may be inflamed by adding a second quantity of oxygen gas, and passing an electric spark.* In this way all the hydrogen of the volatile alkali may be saturated with oxygen, and condensed into water, and the whole of the nitrogen may be obtained as a final result of the process. After determining the amount of the oxygen consumed both in the first and second combustions, it is easy to calculate the quantity of hydrogen in the ammonia which is has been employed; for when no nitrous acid has been formed, the hydrogen will be, pretty exactly, double in volume the oxygen which has been expended.

These general observations will tend to render the following experiments more intelligible. They may be divided into two classes, 1st, those in which ammonia was fired with an excessive proportion of oxygen; and 2dly, those in which the oxygen, used in the first combustion, was insufficient, or barely adequate, to saturate the whole hydrogen of the alkali.

I. *Decomposition of Ammonia by an Excess of oxygen Gas.*

Twenty-two measures and a third of ammonia were mixed with $44\frac{2}{3}$ oxygen containing 43 of pure gas. The total 67 became 34 when exploded. Water did not produce any farther diminution, but sulphuret of lime left only 8 measures. Now, $34 - 8 = 26$ shews the quantity of oxygen gas, which escaped condensation; and this, deducted from the original quantity (43) gives 17 measures for the amount of the oxygen expended. The last number 17, being multiplied by 2, gives 34 for the hydrogen apparently consumed. The final residue $8 - 1.66$ (the nitrogen introduced by the oxygen gas) $= 6.34$ is the nitrogen obtained from $22\frac{1}{3}$ of ammonia; and if to this the hydrogen be added, 40.34 measures of permanent gas will be the total result. Hence 100 measures of the gas producible from ammonia, should contain 84.29 hydrogen and 15.71 nitrogen; numbers too remote from those, which have been already assigned, to be considered even as approximations to the truth. The error arises from the combination of oxygen, during combustion, not only with the hydrogen, but with the nitrogen of the alkali; the latter of which consequently appears deficient, and the former proportionably in excess.

Frequent repetitions of this combustion, with a considerable excess of oxygen gas, continued to give a deficient proportion of nitrogen; and as no accurate conclusions can be drawn from experiments of this kind, I shall proceed to those of the second class.

II. *Experiments, in which Ammonia was fired with a deficient Proportion of oxygen Gas.*

Sixty-three measures of ammonia were exploded over mercury with 33 of oxygen gas containing one of nitrogen. The total 96, when fired by an electric spark, were diminished to 57 measures, which were not contracted any farther by successive agitation with water, and with sulphuret of lime. The whole of the ammonia, therefore, was decomposed; and all the oxygen had entered into combination with the hydrogen of the alkali. The residuary 57 measures were mingled with 40 measures of the same oxygen gas, and detonated by an electric spark ; after which the total, 97, were reduced to 60. The diminution, therefore, was 37 measures; and as two thirds of this number may be ascribed to the condensation of hydrogen gas, the residuary 57 must have been composed of 24.66 hydrogen, and 32.34 nitrogen. The oxygen expended, also, was 32 in the first combustion, +12.33 in the second = 44.33; and this number, being doubled, gives 88.66 for the whole hydrogen saturated, supposing it to be in the state of hydrogen gas. But from the above quantity of nitrogene (32.34 measures) are to deduct one measure, with which the 33 of oxygen were contaminated ; and the remainder number of measures of nitrogen resulting from the decomposition of ammonia. The total amount of gases

obtained is 31.34 + 88.66 = 120; and the proportion of the hydrogen by volume to that of the nitrogen, as 73.88 to 26.12.

To avoid the tediousness of similar details, I shall state, in the form of a table, the results of a few experiments out of a number of others, all of which had, as nearly as could be expected, the same tendency. The sixth experiment in the table is the one which has been just described.

No. of Exp.	Meas. of Ammon. decomposed.	Meas. of Oxygen saturated	Meas. of Hydrogen estimated	Meas. of Nitrogen obtained	Hence 100 Meas. of Ammonia.		Permanent Gas contains in 100 Measures.	
					Take Oxygen	Give Gas.	Hydr.	Nitr.
1	72	47.5	95	37	66	183	72	28
2	95	64	128	46	67.5	183.3	73.5	26 5
3	100	72.2	144.4	54	72.2	198.5	72.8	28.2
4	74	51.7	103.4	37	69 8	189	73.6	27.4
5	49	33.7	67.4	25.3	68 7	180.2	72.7	27.3
6	63	44.3	88.6	31.3	70.3	193.5	73.9	26.1

From an attentive examination of the foregoing table, it will appear that the results are not perfectly uniform, though perhaps as much as can be expected from the nature of the experiments. Thus the proportion of permanent gases to the ammonia decomposed (the nitrogen being actually measured, and the hydrogen estimated by doubling the oxygen expended) may be observed to differ considerably; the highest product being 198½, and the lowest 180.2, from 100 of ammonia.

There can scarcely be a doubt, however, that this want of coincidence is owing to the same cause, as that which I have already assigned for the variable proportions of permanent gas, which are obtained from equal quantities of ammonia by electrization. And, accordingly, I have found that the evolved gases, as ascertained by combustion, bear the smallest proportion to the ammonia, when most pains have been taken to obviate the presence of moisture. The lowest number, therefore, is to be assumed as most correct; but other circumstances being considered, I believe the second experiment furnishes the most accurate data for determining the composition of ammonia. The same explanation will apply to the different proportions of oxygen gas required for the saturation of 100 measures of ammonia, the variation no doubt arising from the uncertainty of the quantity of alkaline gas which is actually burned. The proportion of oxygen to ammonia, which I believe to be nearest the truth, and most precisely necessary for mutual saturation, is that resulting from the second experiment, *viz.* $67\frac{1}{2}$ measures of oxygen gas to 100 of ammonia, or 100 of the former to 148 of the latter.

'It may be observed, also, by comparing the numbers in the two last columns of the table, that the hydrogen and nitrogen gases do not uniformly bear the same proportions to each other. Notwithstanding all the labour I have bestowed on the subject, I have not been able to obtain a nearer correspondence, owing most probably to the imperfection of the mode of analyzing a mixture of hydrogen and nitrogen gases. In the case of permanent gases, determined in this way, the quantity hereindicated, bears generally rather a less amount. I shall hereafter consider this

fact as contradicting the accuracy of the proportions which you have assigned; and it appears to me that a sufficient reason may be given for the want of a more perfect coincidence between results, obtained by such different methods of investigation. In the products of the electrization of ammonia, the hydrogen composes nearly three fourths of the mixture; and hence its combustion by oxygen gas is likely to be completely effected, and the whole of the hydrogen condensed into water. But after the *partial* combustion of ammonia, by oxygen gas, a residuum is left of hydrogen and nitrogen gases, of which the hydrogen usually composes less, and sometimes considerably less, than one half the bulk. In this case, it may be suspected that a small quantity of hydrogen occasionally escapes being burned; and whenever this happens, its proportion to the nitrogen will appear to be less than the true one.*

From the inflammability of a mixture of ammonia with oxygen gas, it was natural to expect that this alkali would prove susceptible of *slow* combustion. By means of a peculiar apparatus (on a plan which I have described in the Philosophical Transactions for 1808, part II. but on a smaller scale, and with the substitution of mercury for water), I have found that ammonia, expelled from the orifice of a small steel burner, may be kindled by electricity in a vessel of oxygen gas; and that it is slowly consumed with a pale yellow

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* This consideration suggests the propriety of using no more oxygen in the first combustion of ammonia, than is barely sufficient to inflame it; or if a larger quantity has been used than is required for this purpose, and a residue consequently obtained, of which the hydrogen forms only a small proportion, it is proper to add a further quantity of hydrogen, before the second combustion. An allowance may, afterwards be made for this addition.

flame.   The combustion, however, is not sufficiently vivid to render the process of any use in the analysis of ammonia.

With nitrous oxide (containing only 5 per cent. impurity) ammonia forms a mixture which is extremely combustible. If the nitrous oxide be in excess, the proportions have a considerable range; for any mixture may be fired by electricity, of which the ammonia is not less than one-sixth of the whole.   The combustion is followed by a dense cloud, sometimes of an orange colour.   When the nitrous oxide greatly exceeds the ammonia, (as in the proportion, for example, of 100 to 30) there is little or no diminution after firing : and the residuum is composed of a small portion of undecomposed oxide, some oxygen gas, and a considerable quantity of nitrogen, the last of which, however, is not in its full proportion. When the nitrous oxide is further increased, still more oxygen is found in the residuum.

When, on the contrary, the alkaline gas is redundant, combustion does not take place unless the nitrous oxide forms one-third of the mixture.   A little diminution takes place on firing, but no cloudiness is observed ; and the residue is composed of hydrogen and nitrogen gases, with occasionally a small portion of undecomposed ammonia.   As an example of what takes place, I select the following experiment from several others.

A mixture of 41 measures of ammonia, with 40 of nitrous oxide (= 38 pure), in all 81 measures, were reduced by combustion to 75, which were found to consist of 16 hydrogen [and ...] nitrogen gases.   To explain this experiment, we may [...] consistent with your own analysis.*) that 100

measures of nitrous oxide, are equivalent to 52 measures of oxygen gas and 103 of nitrogen. The oxygen in 38 mea‑sures of nitrous oxide will, therefore, be 19,7, to which, when the oxygen spent in burning the residuum (viz. 8 m. is added, we obtain 27,7 for the total oxygen consumed ; and multiply‑ing by 2, we have 55,4 for the hydrogen saturated. From the residuary nitrogen (59) deduct 39 measures arising from the decomposition of the nitrous oxide + 2 m., mingled with it as an impurity = 41, and the remainder, 18 measures, is the nitrogen resulting from the volatile alkali; and as 41 mea‑sures of ammonia give 55,4 + 18 = 73,4 measures of per‑manent gas, 100 would give 179 measures, in which the hy‑drogen and nitrogen would exist in the proportion of 75,4 to 24,6. From the same facts it may be deduced, that 100 measures of ammonia require for saturation 130 of nitrous oxide = $67\frac{1}{2}$ oxygen gas. The coincidence then, between the results of the combustion of ammonia with nitrous oxide, and those with oxygen gas, confirms the accuracy of both methods of analysis.

NITROUS GAS, which, it appears from your testimony,* does not compose an inflammable mixture with hydrogen, (nor as I am assured by Mr. DALTON, with any of the varieties of car‑buretted hydrogen) may be employed, I find, for the combus‑tion of ammonia. The proportions required for mutual satur‑ation are about 120 measures of nitrous gas to 100 of am‑monia. An excess of the former gas does not give accurate results; since not only the hydrogen of the ammonia, but some of its nitrogen is also condensed; and the mixture, after being fired, exhibits the cloudy appearance usual in that case.

* Researches, p. 136.

Forty eight measures of ammonia, being fired with 60 nitrous gas, ($=$ 58 pure) both gases were completely decomposed; and a residue left consisting of 61 nitrogen and 9 hydrogen. Sixty measures of ammonia and 41 nitrous gas ($=$ 36,1 pure) gave, after firing, a mixture composed of 10 ammonia, $53\frac{1}{2}$ nitrogen, and $30\frac{1}{2}$ hydrogen. But taking for granted that 100 measures of nitrous gas, according to your analysis, hold in combination a quantity of oxygen equal to $57\frac{1}{2}$ measures of oxygen gas, and of nitrogen equal to $48\frac{1}{2}$ measures, and assuming the proportions of the nitrogen and hydrogen in ammonia, to be those established by your experiments and my own. It will appear from an easy calculation, that the proportion of nitrogen, in the above residua, a little exceeds, and that of the hydrogen rather falls short of what might have been expected. I have not yet been able to reconcile these differences, by the numerous trials required in a process of so much delicacy; and I reserve the enquiry for a season of more leisure. The foregoing statement, I wish to be considered as merely announcing the general fact of the combustibility of a mixture of ammonia and nitrous gas, a property which chiefly derives importance, from its being capable of application to a new method of analysing the latter.

Before concluding this letter, I shall briefly state the results of some experiments, which I have lately made in conjunction with Mr. DALTON, on a subject that formerly occupied my attention; viz. the effect of electricity on the aeriform compounds of carbon and hydrogen. Subsequent

reflection, as well as the candid and judicious criticisms of various writers,* have influenced me to doubt of the accuracy of a few of the conclusions drawn from my former inquiries.† The knowledge of this class of bodies has, also, been so materially advanced during the last twelve years, that the examination of their properties may now be undertaken, with much greater confidence and success than formerly. It is to be lamented, indeed, that experimentalists do not oftener retrace their labours, with the combined advantages of acquired skill, and of a more improved state of the science which they investigate.

The gases, submitted by Mr. DALTON and myself to the action of long continued electrization, were carburetted hydrogen from pit-coal of the specific gravity of about 650 (air being 1000) olefiant gas, and carbonic oxide. Each gas was used in as pure a state as possible ; muriate of lime being first introduced into the same tubes in which the gases were electrified, and being withdrawn when it had exerted its full action. Platina wires were used to convey the electric discharges.

When the electrization of carburetted hydrogen or olefiant gases was continued sufficiently long, they were each found to expand, notwithstanding their extreme dryness. No carbonic acid could be discovered in the electrified gas by the nicest tests. When fired with oxygen, it gave less carbonic

* See Berrhollet's Chemical Statics, Eng. trans. Vol. II. p. 454; Murray's Elements of Chemistry, Vol. II. Note G; a letter from an anonymous correspondent in Nicholson's Journal, 8vo. II. 185; and Aikin's Dictionary of Chemistry, I. 251.

† "Experiments on Carbonated Hydrogen Gas, with a View to determine whether Carbon be a simple or a compound body." Phil. Trans. Vol. LXXVII.

acid than the unexpanded gas, and required less oxygen for saturation. Calculating, from the diminished product of carbonic acid, how much gas had been decomposed by electrization, it appeared that the decomposed part, in all cases, was about doubled. The smaller product of carbonic acid from the electrified gas, was sufficiently explained by a deposition of charcoal on the inner surface of the glass tube, too distinct to be at all equivocal, and most abundant from the olefiant gas. No addition whatsoever of nitrogen was made by the electrization. It appears, therefore, that the hydro-carburetted gases, like ammonia, are separated by electrization into their elements, the carbon being precipitated, and the hydrogen evolved in a separate form, and acquiring a state of greater expansion. This change, however, is effected much more slowly, than the disunion of the elements of ammonia.

From a portion of carbonic acid gas, carefully dried by muriate of lime, and electrized with platina conductors, we obtained, after removing the undecomposed gas by caustic potash, a residuum equal to about one twentieth the whole gas which had been employed. It was found on analysis to consist of oxygen and carbonic oxide gases, in such proportions as to inflame on passing an electric spark through it without any addition, and to be thus convertible again into carbonic acid. In the experiments of M. SAUSSURE, jun.* that ingenious philosopher obtained only carbonic oxide by the same operation, owing doubtless to the electricity having been conveyed by conductors of copper, which would become oxidized, and ꞏꞏꞏ the oxygen from being evolved in a separate form. ꞏꞏꞏ electrified with similar precautions, did not

appear to undergo any change. Eleven hundred discharges from a Leyden jar had no effect on a quantity of the gas, equal to about one tenth of a cubic inch. Its bulk, after this process, was unaltered; no carbonic acid could be discovered in it; and there was no decided trace of oxygen gas in the residuum. The carbon, it appears, therefore, which exists in carbonic oxide, must be held combined by an extremely strong affinity.

With sincere esteem and respect, I am,

Dear Sir,

Your faithful and obliged friend,

WM. HENRY.

XXVII. *New analytical Researches on the Nature of certain Bodies, being an Appendix to the Bakerian Lecture for 1808. By* Humphry Davy, *Esq. Sec. R. S. Prof. Chem. R. I.**

1. *Further Inquiries on the Action of Potassium on Ammonia and on the Analysis of Ammonia.*

THE most remarkable circumstances occurring in the action of potassium upon ammonia are the disappearance of a certain quantity of nitrogene, and the conversion of a part of the potassium into potash.

The first query which I advanced in the last Bakerian Lecture, on this obscure and difficult subject, was whether the gas developed in the first part of the process of the absorption of ammonia by potassium is hydrogene, or a new species of inflammable aeriform substance, the basis of nitrogene?

Experiments made to determine this point have proved, as I expected, that the gas differs in no respect from that given out during the solution of zinc in sulphuric acid; or that produced during the action of potassium on water. By slow combustion with oxygene, it generates pure water only, and

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* The account of the principal facts respecting the action of potassium on ammonia, in this communication, were read before the Royal Society, February 2, 1809. The paper was ordered to be printed March 16, 1809. At that time, having stated to the Society that I had since made some new experiments on this matter, and on the subject discussed in the Bakerian Lecture for 1808, I received permission to add them to the detail of these new observations for publication.

its weight, in a case in which it was mixed with atmospherical air, precisely corresponded with that of an equal quantity of hydrogene.

Another query which I put is, has nitrogene a metallic basis which alloys with the metals employed in the experiment?

This query I cannot answer in so distinct a manner; but such results as I have been able to obtain are negative.

I have examined the potassium generated in the process. It has precisely the same properties as potassium produced in the common experiment of the gun-barrel; and gives the same results by combusion in oxygene, and by the action of water.

In cases in which I had distilled the olive-coloured fusible substance in an iron tray, the surface of the tray appeared much corroded, the metal was brittle, and appeared crystallized. I made a solution of it in muriatic acid; but hydrogene alone was evolved.

I distilled a quantity of the fusible substance from 9 grains of potassium in an iron vessel, which communicated with a receiver containing about 100 grains of mercury, and by a narrow glass tube the gas generated was made to pass through the mercury; the object of this process was to detect if any of the same substance, as that existing in the amalgam from ammonia, was formed; but during the whole period of distillation, the mercury remained unaltered in its appearance, and did not effervesce in the slightest degree when thrown into water.

That the nitrogene which disappears in this experiment is absolutely converted into oxygene and hydrogene, and that its elements are capable of being furnished from water, is a

conclusion of such importance, and so unsupported by the general order of chemical facts, that it ought not to be admitted, except upon the most rigid and evident experimental proofs.

I have repeated the experiment of the absorption of ammonia by potassium in trays of platina or iron, and its distillation in tubes of iron more than twenty times, and often in the presence of some of the most distinguished chemists in this country, from whose acuteness of observation, I hoped no source of error could escape.

The results, though not perfectly uniform, have all been of the same kind as those described in page 55. Six grains of potassium, the quantity constantly used, always caused the disappearance of from 10 to 12.5 cubical inches of well dried ammonia. From 5 5 to 6 cubical inches of hydrogene were produced, a quantity always inferior to that evolved by the action of an equal portion of the metal upon water. In the distillation from 11 to 17 cubical inches of elastic fluid were evolved, and from 1.5 to 2.5 grains of potassium regenerated,

The quantity of ammonia in the products, varied from a portion that was scarcely perceptible to one twelfth or one thirteenth of the whole volume of elastic fluid: and it was least in those cases in which the absence of moisture was most perfectly guarded against. Under these circumstances likewise, more potassium was revived; and the unabsorbable elastic fluid, and particularly the hydrogene in smaller proportion.

When the products of distillation were collected at different periods, it was uniformly found that the proportion of nitrogene to the hydrogene diminished as the process advanced.

The first portions contained considerably more nitrogene in proportion, than the gases evolved during the electrization of ammonia, and the last portions less.

I shall give the results of an experiment, in which the gases produced in distillation were collected in four different vessels, and in which every precaution was taken to avoid sources of inaccuracy.

The barometer was at 29.8, thermometer 65° FAHRENHEIT.

6 grains of potassium absorbed 12 cubic inches of well dried ammonia. The metal was heated in a tray of platina, and the gas contained in a retort of plate glass.

5.8 cubical inches of hydrogene were produced.

The fusible substance was distilled in an iron tube of the capacity of 3 cubical inches and half filled with hydrogene, the adaptors connected with the mercurial apparatus contained .8 of common air.

The first portion of gas collected (the heat being very slowly raised, and long before it had rendered the vessel red), equalled 7.5 cubical inches. It contained .6 of ammonia, 7 of the residuum detonated with $4\frac{1}{2}$ of oxygene gas left a residuum of 4.

The second portion, equal to 3 cubical inches, contained no ammonia. 7.2 measures of it, detonated with 3.8 of oxygene, left a residuum of 3.5.

The third portion was equal to 5 cubical inches; at this time the tube was white hot; it contained no ammonia, 8.5 of it detonated with 4.5 of oxygene diminished to 2.5.

The last portion was a cubical inch and half, collected when the heat was most intense. 4.5 measures, with 3.75 of oxy-gene, left a residuum of 2.8.

The iron tube contained, after the experiment, (as was ascertained by admitting hydrogene when it was cool), 2.7 of gas; which seemed of the same composition as the last portion. The adaptors must have contained .8 of a similar gas.

The tube contained potash in its lowest part, and in its upper part potassium, which gave by its action upon water $1\frac{3}{4}$ cubical inch of hydrogene.

As the largest quantity of hydrogene is always produced at that period of the process, in which the potassium must be conceived to be regenerated, and in which the gases being in the nascent state, its power of action upon them would be greatest, it occurred to me, that if nitrogene was decomposed in the operation, there would probably be a larger quantity of it destroyed by the distillation of the fusible substance, with a fresh quantity of potassium, than by the distillation of it in its common state. On this idea I made several experiments; the results did not differ much from each other, and were such as I had expected. I shall describe one process made with the same apparatus as that which I have just detailed.—Barometer was at 29.5, thermometer 70° FAHRENHEIT.

6 grains of potassium were employed in an iron tray; 10 cubical inches of ammonia were absorbed, a small globule of metal remained unconverted into the fusible substance. A fresh piece of potassium, weighing six grains, was introduced into the tray . . . .

The iron tube and the adaptors (having together a capacity equal to 4.3 cubical inches) contained common air.

was eight cubical inches. 10.25 of it detonated with 3.5 of oxygene diminished to 8.

The second portion equalled five cubical inches; 9⅓ of this, with 5 of oxygene, left a residuum of 3¾.

Of the third portion, 2 cubical inches and ⅓ came over. 9 of it, detonated with 5 of oxygene gas, left a residuum of 1.4.

The iron tube and the adaptors contained, at the end of the experiment, as was proved by cooling and the admission of hydrogene 2.3 cubical inches of gas, which appeared of the same composition as the third portion. Nearly 7 grains of potassium were recovered.

A comparison of these results, with those stated in the preceding page, will fully prove, that there is a much smaller proportion of nitrogene to the hydrogene, in the case in which the olive-coloured substance is distilled with potassium, than in the other case, and there is likewise a larger quantity of potassium converted into potash.

The loss of nitrogene, and the addition of oxygene to the potassium, are sufficiently distinct in both processes; and the want of a correspondence between these results, and those of the experiment detailed in page 55, are not greater than might be expected, when all the circumstances of the operation are considered. In the instance, in which a double quantity of potassium was employed, more potash must have been formed from the oxygene of the common air in the tubes; and the fusible substance, in passing through the atmosphere, absorbs, in different cases different quantities of oxygene and of moisture; during the intervals of the removal of the different portions of gas, likewise, some globules are lost.

In instances when the heat has been more rapidly raised, I

have generally found more potassium destroyed, and less nitrogene in proportion in the aeriform products. In such cases likewise, the loss of weight has been much greater; the gases have been always clouded, and the adaptors, after being exposed to a moist air, emitted a smell of ammonia; from which it seems likely that small quantities of the dark gray substance described in page 50 of this Volume, are sometimes carried over undecompounded in the operation.

In some late experiments, I substituted for the iron tube, a tube of copper, which had been bored from a solid piece, and the sides of which were nearly a quarter of an inch in thickness. My object in using this tube was not only to prevent the heat from being too rapidly communicated to the fusible substance, but likewise to be secure that no metallic oxide was present, for though the iron tubes had been carefully cleaned, yet still it was possible that some oxide, which could not be separated from the welded parts, might exist, which of course would occasion the disappearance of a certain quantity of potassium.

I shall give the results of one of the processes, which I regard as most correct, made in the tube of copper. The barometer was at 30.5; thermometer was at 59° FAHRENHEIT.

The tube contained two cubical inches and half, and was filled with hydrogene.

6 grains of potassium, which had absorbed 13 cubical inches of ammonia in a copper tray were employed.

The adaptors connected with the mercurial apparatus and stop-cocks, contained .7 of atmospherical air.

The gas given off was collected in two portions.

The whole was equal to 11 cubical inches. It con-

tained .8 of ammonia, 11 of the residuum, detonated with 8 of oxygene, left 8.

The second portion equalled 2 cubical inches. They contained no ammonia. 10 of this gas, with 8 of oxygene detonated, left a residuum of 10.

There remained in the tube and adaptors 1.1 cubical inch of gas.

The quantity of hydrogene produced by the action of the potassium, which had been regenerated, equalled 4.5 cubical inches.

In this experiment the heat was applied much more slowly than in any of those in which the iron tube was used, and even at the end of the operation, the temperature was little more than that of cherry red.

In the upper part of the stop-cock there was found a minute quantity of gray powder, which gave ammonia by the operation of moisture.

In no case, in which I have used the copper tube in like processes of slow distillation, has there been less than 4 grains of potassium revived; and the proportion of nitrogene to the hydrogene in the gas evolved has been uniformly much greater than in processes of rapid distillation in the tubes of iron; but the whole quantity of elastic matter procured considerably less.

Copper has a much stronger affinity for potassium * than

* Copper heated in potassium speedily dissolves, and diminishes its fusibility; but potassium requires a white heat to enable it to combine with iron. In another experiment, in which I distilled the fusible substance in an iron tray, contained in the copper tube, a considerable quantity of copper, that had been dissolved, was found in the state of powder deposited upon the tray, or loose in the bottom of the tube.

iron. It occurred to me as probable, that this attraction, by preventing the potassium from rising in vapour at its usual temperature, and likewise by the general tendency of such combination to give greater density, might occasion a diminution of its action upon the nitrogene in the nascent state. Ammonia has a strong attraction for the oxide of copper, and it consequently is not unlikely that the fusible substance may combine with metallic copper, and that this compound may not be entirely destroyed in the distillation. And assuming this, it may be conceived that the loss of hydrogene partly depends upon some combination of the basis of ammonia with copper.

I had a tube, of the capacity of $2\frac{1}{4}$ cubical inches, made of wrought platina, cemented by means of fine gold solder. The fusible substance was obtained (as usual from six grains of potassium) in a tray of platina, where it was brought in contact with a large surface of platina wire; the distillation was slowly conducted; but before the temperature of the tube had approached to that of ignition, it dissolved and gave way at the points where it was soldered, and a violent combustion took place. Only 7 cubical inches of gas were collected; but of this, allowing for the hydrogene that filled the tube, nearly 3 were nitrogene.

I am making preparations for performing the experiment in a tube made of a single piece of platina, and likewise in tubes made of other metals, and I hope to be able, in some time, to have the honour of laying the results before the Society, for bringing forward the present communications at the present time, except because that my

motive for so doing, is the desire of being assisted or corrected by the opinions and advice of the learned chemical philosophers belonging to this illustrious body. In an investigation connected with almost all the theoretical arrangements of chemistry, and in operations of so much delicacy, it will, I conceive, be allowed, that it is scarcely possible to proceed with too much caution, or to multiply facts to too great an extent.

The different phenomena presented by the processes of distillation in different metallic tubes, may lead to new explanations of this intricate subject, and though the facts cannot be easily accounted for, except on the supposition that nitrogene is an oxide, yet till the proportions and weights are distinctly ascertained, the inquiry cannot be considered as far advanced, for in an experiment, in which the processes are so complicated and delicate, and in which the data are so numerous, it is not easy to be satisfied that every source of error has been avoided, and that every circumstance has been examined and reasoned upon.

All conclusions on the action of potassium on ammonia, are immediately dependent upon the results of the electrical analysis of the volatile alkali. In a letter, which I received in the course of the last month from Dr. HENRY, that excellent chemist, has stated that he conceives I have rather under-rated the quantity of nitrogene in ammonia, according to the proportions given in the Bakerian Lecture for 1807. This notice has induced me to repeat the experiment, under new circumstances, and I find not the slightest reason for doubting of the entire accuracy of my former results.

In the new trial, I used mercury which had been recently

boiled in the tube for electrization; the ammonia was intro-
duced after being long dried by caustic potash, from a receiver
in which it had not been generated, and which had likewise
been inverted over boiling mercury. The gas left no percep-
tible residuum, when absorbed by water deprived of air by
boiling. In this process, 15 measures of ammonia expanded,
so as to fill 27 measures; and the hydrogene by detonation
with oxygene, over water freed as much as possible from air,
proved to be to the nitrogene as 73.8 to 26.2. In the experi-
ment three explosions were made, the oxygene being deficient
in the first two; so that no nitrogene could have been con-
densed in the form of nitric acid.

Except when precautions of this kind are employed, as I
have before noticed, no accurate data can be obtained re-
specting the proportions of permanent gases obtained from
ammonia by electricity.

When the gas is generated and decomposed over the same
mercury, there is always a greater expansion than the true
one; and when the mercury is not boiled in the tube, and
when common water is used, the nitrogene will be always
over-rated, unless this error is counteracted by an opposite
error, that of detonating with an excess of oxygene.*

Dr. HENRY had the kindness to send me the apparatus, in
which he conceived, at that time, that he had witnessed the
formation of water in the decomposition of ammonia by elec-

* It will be seen by Dr. HENRY's letter, which immediately precedes this com-
munication, that in repeating his processes, since this paper was written, he has
obtained results almost precisely the same as those indicated in the text; and there is
every reason to believe, that 100 of ammonia in volume uniformly become 180, when
decomposed by electricity, and that the gas produced consists in 100 parts of 74

tricity, by his ingenious method of applying hygrometrical tests.

I tried one experiment only with it, and in this there seemed to me to be more moisture exhibited in the elastic matter after electrization than before, when it was cooled by the evaporation of ether: but on maturely considering this question, I do not think that the appearance of moisture even offers a decided proof of the existence of loosely combined oxygene in ammonia. To common hygrometrical tests, water must be less sensible in ammonia than in hydrogene or nitrogene, from its tendency to be precipitated in the form of alkaline solution, and likewise probably from its having a stronger adherence to the gas; and the elastic fluid generated, from the increase of volume will be capable of containing more aqueous vapour.

It is not easy to determine, with perfect precision, the specific gravity of a gas, so light as hydrogene and even ammonia; but the loss of weight, which appears to take place in the electrical analysis of ammonia, cannot, I think, with propriety, be referred entirely to this circumstance; whether the solution that I have ventured to give* be the true one, I shall not, in the present state of the inquiry, attempt to discuss.

The question of ammonia being analogous to other salifiable bases in its constitution, is determined by the phænomena presented by the amalgam from that alkali; and if the conversion of nitrogene into oxygene and hydrogene should be established,* it would appear that both hydrogene and nitrogene must be different combinations of ammonium with oxygene, or with water.

* Bakerian Lecture, 1807, p. 40.

II. *Further Inquiries respecting Sulphur and Phosphorus.*

I have stated, in the last Bakerian Lecture, that hydrogene is produced from sulphur and phosphorus in such quantities, by Voltaic electricity, that it cannot well be considered as an accidental ingredient in these bodies. I have likewise stated, that when potassium is made to act upon them, the sulphurets and phosphurets evolve less hydrogene in the form of compound inflammable gas by the action of an acid, than the same quantity of potassium in an uncombined state, and from this circumstance, I have ventured to infer, that they may contain oxygene.

On the idea, that sulphur and phosphorus are deprived of some of their oxygene by potassium, it would follow, that when the compounds formed in this experiment are decomposed, these substances ought to be found in a new state; deoxygenated, as far as is compatible with their existence in contact with water.

With the view of examining the nature of the substances, separated by the action of muriatic acid upon the sulphurets and phosphurets of potassium, I combined a few grains of sulphur and phosphorus, with one fourth of their weight of potassium, and exposed the compounds to the action of a strong solution of muriatic acid. As in the former cases, less inflammable gas was produced than would have been afforded by equal quantities of the uncombined potassium, and considerable quantities of solid matter separated from both compounds, which after being washed were collected in a fluid.

The substance which separated from the sulphuret, was of

a dark gray colour,* and was harsh to the touch; it had no taste, and at common temperatures no smell; but when heated, it emitted the peculiar odour of sulphur. Its specific gravity was rather less than that of sulphur. It softened at a low heat, so as to be moulded like wax between the fingers. It was a non-conductor of electricity. When heated upon a surface of glass, it soon fused, entered into ebullition, took fire, and burnt with the same light blue flame as sulphur. A small particle of it, made to combine with silver, presented the same phenomena as sulphur.

The substance from the phosphuret was of an amber colour, and opaque. It could not be examined in the air, in the form in which it was collected (that of a loose powder) for as soon as it was wiped dry, it took fire, and burnt in the same manner as phosphorus; when melted under naphtha, it was found to differ from phosphorus, in being much deeper coloured, perfectly opaque, and very brittle. Its fusibility was nearly the same, and, like common phosphorus, it was perfectly non-conducting.

In experiments upon the union of potassium with sulphur and phosphorus the heat is so intense, that when larger quantities than a few grains are used, the glass tubes are uniformly fused or broken in pieces, and in consequence I have not been able to operate upon such a scale, as to make an accurate examination of the substances just described, and to determine the quantity of oxygene they absorb in being converted into acid. Metallic vessels of course cannot be employed; but I

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* Possibly this colour may have been produced by the decomposition of a film of soap of naphtha adhering to the potassium.

intend to try tubes of porcelain, in a further investigation of the subject.

It is evident that the sulphur and phosphorus, separated in these processes, are not in their common state; and the phenomena would certainly incline one to believe that they ar less oxygenated. It may, I know, be said, that it is possible that they are merely combined with more hydrogene, and that the sulphur in this state is analogous to the hydrogenated sulphur of BERTHOLLET, and to the alcohol of sulphur of LAMPADIUS.

But when I decompounded dry sulphuret of potash by muriatic acid, of the same kind as had been used for decompounding the sulphuret of potassium, the substance produced seemed to be merely in that form, in which, according to the able researches of Dr. THOMSON, it is combined with water; and notwithstanding the ingenious experiments of M. A. BERTHOLLET and M. ROBIQUET,* the nature of the substance produced during the passage of sulphur over ignited charcoal is far from being fully ascertained. In a series of experiments, which my brother, Mr. JOHN DAVY, had the goodness to undertake, at my request, in the laboratory of the Royal Institution, on the action of sulphur on charcoal, the products were found to be very different, according as the charcoal employed differed in its nature. In an instance, in which imperfectly made charcoal was employed, the liquor that passed over left by combustion a residuum that had all the properties of carbonaceous matter, which agrees with the observations of M. M. DESORMES and CLEMENT; but when the charcoal had been well burnt, there was such residuum produced. It was found, that the same

charcoal might be employed in a number of processes till it was nearly entirely consumed, and that the sulphur, not rendered liquid, might be used for several operations. In all cases mixtures of * sulphuretted hydrogene gas and hydrocarbonate were evolved.

I particularly examined a specimen of the liquor which had been obtained in the last process from charcoal that had been often used. It was a non-conductor of electricity, and when the VOLTAIC spark was taken in it, did not evolve gas with more rapidity than sulphur; and this gas proved to be sulphuretted hydrogene.

Supposing the liquor to contain hydrogene in considerable quantities, I conceived that it must be decomposed by oxymuriatic acid; but it merely absorbed this substance, depositing crystals of common sulphur, and becoming a fluid similar to the sulphuretted muriatic acid; though when water was introduced, hydrated sulphur was instantly formed, and muriatic acid gas evolved.

From the quantity of carbonic acid formed by the combustion of the carburetted inflammable gas, produced in the operation of the action of well burnt charcoal upon sulphur, it may be conceived to contain oxygene. This circumstance, and the fact that no hydrate of sulphur or muriatic acid gas is formed by the operation of oxymuriatic acid upon the liquor, but common sulphur precipitated; are in favour of the opinion, that the sulphur in this liquor, contains less oxygene than in its common state. This idea has likewise occurred to

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* Five measures of the mixed gas, agitated with solution of potash, left a residuum of 3.5. These were detonated with 5.5 of oxygene; the whole diminution, was to 6, of this residuum 2.5 appeared to be carbonic acid.

Dr. MARCET, who is engaged in some experiments on the subject, and from whose skill and accuracy, further elucidations of it may be expected.

III. *Further Inquiries respecting carbonaceous Matter.*

On the idea which I have stated, page 73, that the diamond may consist of the carbonaceous matter combined with a little oxygene, I exposed charcoal intensely ignited, by VOLTAIC electricity,* to nitrogene, conceiving it possible that if this body was an oxide, containing oxygene very intimately combined, it might part with it in small proportions to carbonaceous matter, and give an important result.

The charcoal, which had been made with great care, was preserved for a quarter of an hour in a state of ignition, in which platina instantly fused. It did not appear to change in its visible properties; but a small quantity of black sublimate, which proved to be nothing more than finely divided carbonaceous matter, collected in an arborescent state upon the platina wire to which the charcoal was attached. The gas had increased in volume one sixth; but this was owing to the evolution of carburetted inflammable gas from the charcoal, the nitrogene was unchanged in quantity, and far as my examination could go, in quality. The points of the charcoal where the heat had been intense, were rather harder than before the experiment.

I have mentioned, page 102, that charcoal, even when strongly ignited, is incapable of decomposing corrosive sub-

contact with oxymuriatic acid gas, the combustion instantly ceases. I electrified two pieces of charcoal in a globe filled with oxymuriatic acid gas, which had been introduced after exhaustion of the globe. They were preserved, for nearly an hour, in intense ignition, by the same means that had been employed in the experiment on nitrogene. At first, white fumes arose, probably principally from the formation of common muriatic acid gas, by the action of the hydrogene of the charcoal upon the oxymuriatic acid, and the combination of the gas so produced, with aqueous vapour in the globe; but this effect soon ceased. At the end of the process, the oxymuriatic acid gas was found unaltered in its properties, and copper leaf burnt in it with a vivid light. The charcoal did not perceptibly differ from the charcoal that had been exposed to nitrogene. My view in making this experiment, was to ascertain whether some new combination of carbonaceous matter with oxygene might not be formed in the process, and I hoped likewise to be able to free charcoal entirely from combined hydrogene, and from alkaline and earthy matter, supposing they existed in it, not fully combined with oxygene. That hydrogene must have separated in the experiment, it is not possible to doubt, and on evaporating the deposit on the sides of the globe, which was in very minute quantity, and acted like concentrated muriatic acid, it left a perceptible saline residuum.*

* Charcoal, over which sulphur has been passed, as in the experiments, page 465, as has been shewn by M. A. BERTHOLLET, contains sulphur, and this I find after being heated to whiteness; such charcoal is a conductor of electricity, and does not differ in its external properties from common charcoal.

IV. *Further Inquiries respecting muriatic Acid.*

The experiments on muriatic acid, which I have already had the honour of laying before the Society, shew that the ideas which had been formerly entertained respecting the difference between the muriatic acid and the oxymuriatic acid are not correct. They prove that muriatic acid gas is a compound of a substance, which as yet has never been procured in an un-combined state, and from one-third to one-fourth of water, and that oxymuriatic acid is composed of the same substance, (free from water) united to oxygene. They likewise prove, that when bodies are oxydated in muriatic acid gas, it is by a de-composition of the water contained in that substance, and when they are oxydated in oxymuriatic acid, it is by combina-tion with the oxygene in that body, and in both cases there is always a union of the peculiar unknown substance, the dry muriatic acid with the oxydated body.

Of all known substances belonging to the class of acids, the dry muriatic acid is that which seems to possess the strongest and most extensive powers of combination. It unites with all acid matters that have been experimented upon, except car-bonic acid, and with all oxides (including water), and all in-flammable substances that have been tried, except those which appear to be elementary, carbonaceous matter and the me-tals; and should its basis ever be separated in the pure form, it will probably be one of the most powerful agents in che-mistry.

I have lately made several new attempts to procure un-combined dry muriatic acid; but they have been all unsuc-cessful.

I heated intensely, in an iron tube, silex in a very minute state of division, and muriate of soda that had been fused; but there was not the smallest quantity of gas evolved. In this case, the silex had been ignited to whiteness before it was used; but when silex in its common state was employed, or when aqueous vapour was passed over a mixture of dry silex and dry salt in a porcelain tube, muriatic acid gas was developed with great rapidity.

I have stated, page 79, that a sublimate is formed by the combustion of the olive-coloured oxide of boracium in oxy-muriatic acid. On the idea that this might be boracic acid, and that dry muriatic acid might be separated in the process, I examined the circumstances of the experiment; but I found the sublimate to be a compound of boracic and muriatic acid, similar to the compound of muriatic and phosphoric acid.

I heated freshly sublimed muriate of ammonia with potassium; when the quantities were equal, as much hydrogene gas was developed as is generated by the action of water on potassium; much ammonia was evolved, and muriate of potash formed; when the potassium was to the muriate as 4 to 1, less hydrogen appeared, and a triple compound of muriatic acid, ammonia, and potassium, or its protoxide was formed, which was of a dark gray colour, and gave ammonia and muriate of potash by the action of water. There was not the slightest indications of the decomposition of the acid in the experiment. The process, in which this decomposition may be most reasonably conceived to take place, is in the combustion of potassium in the phosphuretted muriatic acid, deprived

by simple distillation with potassium of as much phosphorus as possible. I am preparing an apparatus for performing this experiment, in a manner which, I hope, will lead to distinct conclusions.

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1808.	PRESENTS.	DONORS.
Nov. 10.	Proceedings of the Commissioners for the arrangement and preservation of the Public Records of the Kingdom, 1806—1808, so far as relates to Scotland. fol.	The Commissioners of the Public Records.
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	The Gospel best promulgated by national Schools, a Sermon by F. Wrangham. York, 1808. 4°	The Rev. Francis Wrangham, M. A. F. R. S.
	The Philosophical Transactions abridged. Vol. XV. Part IV. and Vol. XVI. Parts I. II. III.	Messrs. C. and R. Baldwin.
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15. A Journal of Natural Philosophy, by W. Nicholson. No XCIV.	Mr. William Nicholson.

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Jan. 12. Mémoires de la Classe des Sciences Mathematiques et Physiques de l'Institut National de France 1 Semestre de 1807. Paris, 1807. 4°	The National Institute of France.
Mesure de l'Arc du Meridien compris entre les Paralleles de Dunkerque et Barcelone, par M. M. Mechain et Delambre, Tome II. Paris, 1807. 4°	
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The Critical Review. December, 1808.	The Proprietor.
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The Philosophical Magazine, by A. Tilloch. No. CXXIX.	Mr. Alexander Tilloch.
The Critical Review. January, 1809.	The Proprietor.
16. Traité de Mineralogie, par M. le Comte de Bournon. Londres, 1808. 3 Vols. 4°	Le Comte de Bournon, F. R. S.
23. Appendix to the Meteorological Journals kept in London by W. Bent. London, 1809. 8°	Mr. William Bent.
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The Critical Review. February, 1809.	The Proprietor.

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16. The Nautical Almanac for the Year 1813. London, 1808. 8° — The Commissioners of Longitude.

A Sermon preached before the Lords, 8 February, 1809, being the day appointed for a General Fast; by Sam. Lord Bishop of Carlisle. London, 1809. 4° — The Lord Bishop of Carlisle, F. R. S.

23. An Essay on the Theory of the various Orders of logarithmic Transcendents, by W. Spence. London, 1809. 4° — Mr. William Spence.

April **13.** The Philosophical Transactions abridged. Vol. XVII. Part IV. — Messrs. C. and R. Baldwin.

The Philosophical Magazine, by A. Tilloch. No. CXXXI. — Mr. Alexander Tilloch.

Essai sur les Médailles plaquées des Anciens, par L. de Waxell. — M. L. de Waxell.

The Critical Review. March, 1809. — The Proprietor.

20. Fasciculus XV. of a Synopsis of the British Confervæ, by L. W. Dillwyn. — Lewis Weston Dillwyn, Esq. F. R. S.

27. Directions for sailing to and from the East Indies, China, &c. by J. Horsburgh. London, 1809. 4° — James Horsburgh, Esq. F. R. S.

Two Charts of the Coasts of Africa and Madagascar, by Capt. P. Heywood.

An Inquiry into the Laws of Epidemics, by J. Adams. London, 1809. 8° — Joseph Adams, M. D.

Meteorological Journal kept at Swan River, Lat. 52° 22' N. Long. 100° 46' W. from Nov 1, 1807 to March 29, 1808, by Peter Fidler. Mscr. fol. — Joseph Colen, Esq.

Opinion delivered by Dr. Duncan, Sen. in the College of Physicians of Edinburgh, 13 Sept. 1808. Edinburgh, 1808. 4° — Andrew Duncan, senior, M. D.

T. Simsoni de Re medica Dissertationes IV. iterum excudi curabat A. Duncan, Sen. Edinburgi, 1809. 8°

May **4.** A Catalogue of the Harleian Manuscripts in the British Museum. 1808. 3 Vols. fol. — The Commissioners of the Public Records.

The Philosophical Magazine, by A. Tilloch. No. CXXXII. — Mr. Alexander Tilloch.

The Critical Review. April, 1809. — The Proprietor.

11. Ly Tang, an imperial Poem, in Chinese, by Kien Lung, with a Translation and Notes, by S. Weston. London, 1809. 8° — The Rev. Stephen Weston, B. D. F. R. S.

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The Critical Review. May, 1809.	The Proprietor.
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15. Transactions of the Royal Society of Edinburgh. Vol. V. Part III. 1805. and Vol. VI. Part II. 1809. 4°	The Royal Society of Edinbugh.
22. Kongl. Vetenskaps Academiens Nya Handlingar Tom. XXVII. for år 1806, 2d, 3d, and 4th quarter, Tom. XXVIII. för år 1807, and Tom. XXIX. for år 1808. Stockholm. 8°	The Royal Academy of Sciences of Stockholm.
A new Elucidation of Colours, by J. Sowerby. London, 1809. 4°	Mr. James Sowerby.

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